

THE EFFECTS OF EPANUTIN AND ELECTROCONVULSIVE SHOCK ON THE MAZE PERFORMANCE OF RATS

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I. INTRODUCTION

The purpose of this study is to determine whether or not the anticonvulsant drug, epanutin,² protects rats from the deleterious effects of a series of electroconvulsive shocks (E.C.S.) on maze learning. The experiment is designed to test the hypothesis⁽¹⁹⁾ that the occurrence of the electrically induced convulsions is a causal factor in the subsequent decrement in maze performance which is observed.

Two types of anticonvulsants have been used in researches of this nature: general anaesthetics and synthetic anticonvulsants. Ether anaesthesia at the time of administering E.C.S. prevents the usual convulsion, and protects the rats from some of the detrimental effects resulting from E.C.S.^(15, 23) Nembutal anaesthesia prevents retardation of the runway performance of rats receiving E.C.S.⁽¹⁹⁾, although in this case the results are difficult to interpret, since it has been shown that under certain conditions the performance of unmedicated animals on this task is not affected by E.C.S.⁽¹⁸⁾.

Epanutin is a powerful synthetic anticonvulsant which is widely used in the control of human epilepsy. It is also effective against audiogenic seizures in rats^(5, 8, 17). However, the data presented by Thomas & Stone⁽²⁴⁾ indicate that, while this drug modifies electrically induced seizures, it does not prevent the impairment in maze learning attributable to E.C.S. In the present experiment an attempt is made to confirm or refute the Thomas & Stone findings and to extend the controls used to allow the separate evaluation of the effects of the two independent variables, epanutin and E.C.S., on the dependent variable, maze learning.

II. PROCEDURES

Maze. An eight-choice water maze of the Warden-U type was used. The maze is of the same general plan as that described by Porter & Stone⁽¹⁵⁾, but longer by one unit. The correct sequence of choices is LRRRLRLR. It is constructed of galvanized iron, with alleys 5 in. wide and 14 in. deep. It is set in a large tank in which the water is maintained at a depth of 6 $\frac{3}{4}$ in. The water temperature during the experiment ranged from 15 to 21° C., with a mean of 17.5° C.; the air temperature ranged from 19.5 to 35° C., with a mean of 26° C.

Subjects. Seventy experimentally naïve albino rats (thirty-eight males and thirty-two females) of the Wistar strain were used. Though this strain has been maintained at Stanford University for about 50 years, it has not been selectively bred to produce a genetically pure line. The mean age of the subjects at the beginning of the experiment

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² Phenytoin sodium or sodium diphenylhydantoinate; also known, in the U.S.A., as dilantin sodium or dilantin.

was 124 days; the mean weight of the females was 193 g., and, of the males, 243 g. Two further animals died during the course of the experiment; all their data were discarded.

Procedure. All animals were handled daily until they were thoroughly tame. On six successive days they were given one trial swim in a 10 ft. straight alley that is situated in the maze tank and has the same directional orientation as the maze. On the next day, the first of three preliminary trials, one per day, in the maze was given. Gates were used in all trials at the entrance to the 2nd, 4th, 6th and 8th units to prevent retracing. After trial 3, the animals were randomly assigned to the experimental groups shown in Table 1. The only limitation on this random assignment was the maintenance of approximately equal numbers of males and females in each group. For 12 days—the treatment phase—these groups were given the treatments indicated in Table 1 as well as one trial a day in

Table 1. *Groups during treatment phase*

Designation	Treatment	Composition		Total
		Males	Females	
Shock groups DS dS	Drug and shock	9	9	18
	Placebo and shock	9	9	18
Non-shock groups Ds ds	Drug and pseudo-shock	10	7	17
	Placebo and pseudo-shock	10	7	17
	Total	38	32	70

the maze. After the treatments ceased, twelve further trials were given. Thus the twenty-seven trials in the maze comprise:

- (a) Days 1 to 3 incl.: Preliminary trials;
- (b) Days 4 to 15 incl.: Treatment phase;
- (c) Days 16 to 27 incl.: Post-treatment phase.

Epanutin treatments. Intraperitoneal injections were used. Preliminary work showed that a dosage of 75 mg. of epanutin per kg. of body weight administered by this method modified E.C.S. seizures without apparently injuring the rats' health or affecting their ability to swim(4). The crystalline epanutin was made up in the form of a solution in water suspension, so that the injection of 1 c.c. of fluid per 100 g. of body weight would yield the chosen dosage. In order to equalize any emotional or physiological reaction resulting from the injection, an equal amount, i.e. 1 c.c./100 g., of a placebo was given to the groups not receiving the drug. Ringer's solution was used for this purpose as it appears to have no effect on the threshold for electrically induced seizures(25).

Electroconvulsive shocks. The Pittsburgh Electroshock Unit(16) was used to deliver a E.C.S. of 30 mA. for 0.2 sec. Alligator clips with platinum electrodes soldered to the jaw were attached to the rat's ears for the purpose. The current is adequate to produce a tonic-clonic convulsion in unmedicated rats on nearly every occasion. On the few occasions when it failed to do so, the E.C.S. was repeated after 30 min. had elapsed. Pseudo-shock—a term designating treatment similar to E.C.S. but without the actual flow of any current—was given to the two groups not receiving E.C.S.

The time sequences involved were as follows. A few minutes after the completion of each of trials 3–14 inclusive, each group of rats was injected with epanutin or the placebo. Necessarily this introduced some variations in the trial-to-injection interval for different members of a group, since all members of the group in question completed their maze trial before any were injected. These differences, though ranging on rare occasions from 2 to 58 min., were relatively constant, since the animals were tested on the maze in the

same order each day. The interval between injection and E.C.S. or pseudo-shock was standardized at 10 min., a convenient time interval which has been found to be adequate for the epanutin to cause its typical effects (4). The interval between daily trials was also constant. About $23\frac{1}{2}$ hr. elapsed between one day's treatments and the following day's maze trial; thus each trial on days 4-15, inclusive, was about $23\frac{1}{2}$ hr. after medication and/or E.C.S. This time interval was chosen, as it was desired to test cumulative effects of a series of electroconvulsive shocks rather than their immediate confusional effects.

Scores. Learning was measured in terms of the records on a fixed number of trials. The following measures were obtained:

(a) *Forward-going errors.* The number was recorded of any blind alley that the animal's body, including tail, entered.

(b) *Retracing errors.* The number of any blind alley that the animal entered when swimming a pathway that would take it toward the start rather than lead it to the goal was also noted.

(c) *Time.* The time elapsing from the moment the rat was dropped into the water at the start till both forefeet were placed on the ramp leading to the goal platform was recorded.

Multiple entries into a blind alley were counted as separate errors.

III. RESULTS

(i) *Effects of epanutin*

General activity. A few minutes after receiving an interperitoneal injection of 75 mg. of epanutin per kg. of body weight, rats usually cease exploratory activity and become immobile. Occasionally an animal will lie on its side; more frequently it will lie on its belly with head extended forward, often with a characteristic backward extension of the hind-legs. This posture is markedly different from the normal crouch. At this time any strong auditory stimulus will evoke an investigatory response, and a slight tremor of the head accompanying this movement may be detected. Ataxia is rarely seen with this dosage. If left undisturbed the rat will sleep for an hour or more.

Anticonvulsant effects. A marked modification of electrically induced seizures is apparent. Instead of the typical tonic-clonic reaction (14) which results from E.C.S. of 30 mA. for 0.2 sec., a range of reaction patterns appears. They can be broadly grouped into two, which, with Golub and Morgan (7), will be termed 'missed fits' and 'racing seizures', although in no case did the reaction end in a coma. The majority of the seizures in the rugged animals (DS group) were of the missed fit type (see Table 2). Here the sequence

Table 2. *Reactions to electroconvulsive shocks*

Group	Tonic-clonic	Racing seizure	Missed fit	Total
Drug-shock (DS)	7 (3.2)*	61 (28.2)	148 (68.5)	216
Placebo-shock (dS)	211 (97.7)	5 (2.3)	0 (0)	216

* The figures in brackets give the percentage of the total which the adjacent figure represents.

an initial start, as in a tonic-clonic seizure, during which the animal may fall on one side momentarily. This may be the extent of the reaction, but, more typically, a short run across the cardboard tray in which the animal was shocked follows and terminates the reaction. Recovery is illustrated in Fig. 1.

A racing seizure may be characterized as a more massive reaction. The initial start is occasionally followed by some hypertonus, but most characteristic is the violent, undirected running which soon follows. The rat frequently falls from the table to the floor in the process, and the hyperactivity may continue for as long as 30 sec. The running frequently stops when the animal meets with an obstacle, after which it stays crouched and quiet until picked up. Associated with the running may be repeated hopping like that seen in some audiogenic seizures (10), and/or repeated vocalization lasting for as long as 20 sec.

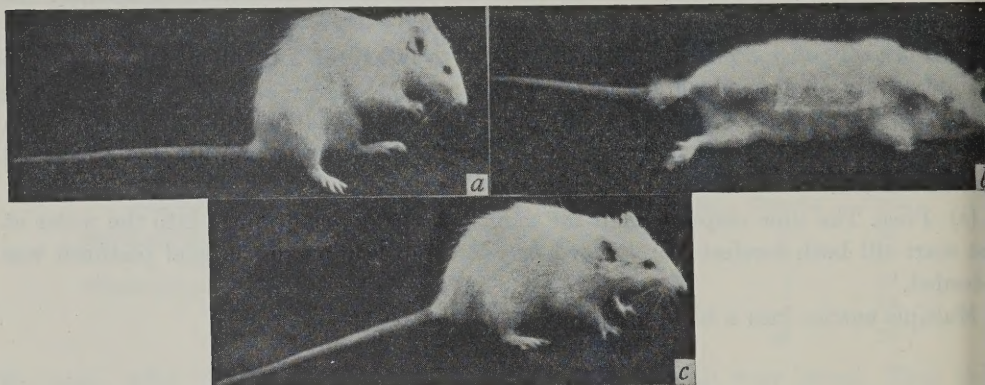


Fig. 1. Recovery from E.C.S. with and without epanutin. *a* shows a female rat from the DS group, 22 sec. after the standard E.C.S. The shock, administered 10 min. after an injection of 75 mg. of epanutin per kg. of body weight, was followed by a 'missed fit' (see text) lasting about 6 sec. *b* shows a female rat from the dS group 22 sec. after the standard E.C.S. This animal is in the clonic phase of the tonic-clonic convulsion which resulted from the E.C.S., as evidenced by movement of the right hindleg which was too fast for perfect definition with the exposure used ($\frac{1}{50}$ sec.). *c* shows the animal of Fig. 1*a*, 25 sec. after the *a* photograph, i.e. 47 sec. after the E.C.S. Normal posture and appearance have already returned. (Recovery in unmedicated rats takes at least 90 sec. (14).)

The reaction of each subject in the shock groups to each of the series of twelve E.C.S. was thus classified, and the frequencies of each type are given in Table 2. It should be noted that each subject is represented twelve times. These classifications are necessarily subjective and possibly unreliable, and the absolute value should therefore be accepted with reserve. However, it is apparent from an inspection of the table that the relative frequencies are unlikely to have arisen by chance. It would seem that there is a real effect which can be attributed to epanutin.

Weight. All the animals were weighed before each maze trial. The resulting data when plotted against trials show little of interest. A general upward trend over the whole period of the experiment is interrupted for a few days at the beginning of the treatment phase, but by the time the treatments ceased all groups weighed more than before the experiment began. Despite the random assignment of subjects to groups, there were no differences in the mean weights at the outset of the experiment. However, the differences were, in general, maintained throughout, and there appears to be no evidence of any pronounced effect of epanutin (or of E.C.S.) on weight.

(ii) *Quantitative measures of learning*

Figs. 2*a* and *b* present the group means for errors and for time scores on successful daily trials. For further analysis the data were divided into two sections: those from the treatment phase (trials 4–15, inclusive), and those from the post-treatment phase (trials 16–25, inclusive).

16-27, inclusive). Each subject was assigned a value representing, for example, his total forward-going errors made during each phase. The retracing errors and the time scores were treated in the same manner. Table 3 shows the group means and estimated population variances of these totals for the three measures.

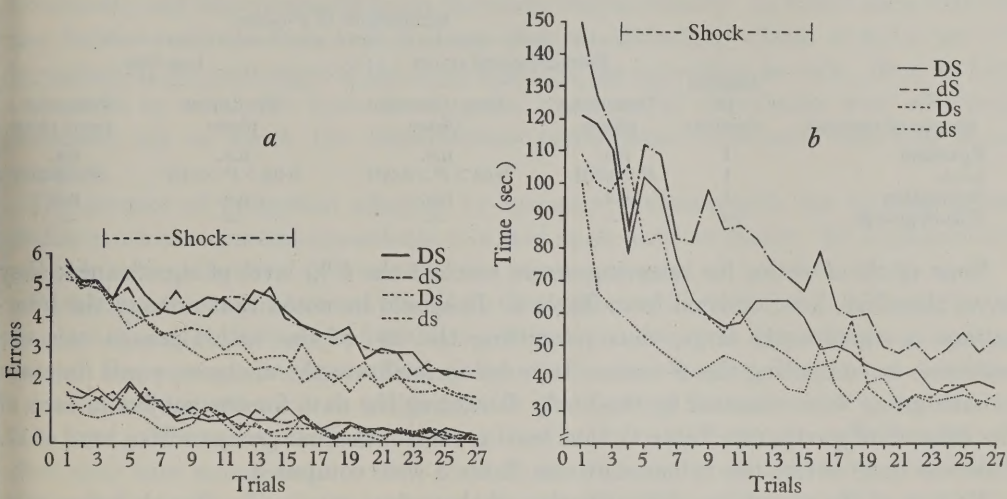


Fig. 2. *a*. Group means for forward-going and retracing errors. The *upper curves* show the mean forward-going errors for each of the four groups throughout the experiment. It will be observed that the learning of the two shock groups (DS and dS) is markedly retarded during the treatment phase in comparison with the non-shock groups (Ds and ds), but that some recovery takes place after the treatments are discontinued. The *lower curves* show the mean retracing errors for each group throughout the experiment. There is little evidence of any differentiation in retracings between the groups. *b*. Group means for time scores. The curves show the mean time scores for each of the four experimental groups throughout the experiment. Despite the random assignment of subjects to groups, there are marked differences between the scores made on the training trials. Thereafter a general tendency for the non-shock groups (Ds and ds) to do better than the shock groups (DS and dS) is apparent. The high mean values for the dS group are partly due to the presence of two very slow swimmers in this group.

Table 3. *Results*

Measure	Group	Treatment phase		Post-treatment phase	
		<i>M</i>	<i>s</i> ²	<i>M</i>	<i>s</i> ²
Forward-going errors	DS	53.2	155.8	32.4	120.1
	dS	51.7	135.1	31.8	183.8
	Ds	40.6	209.1	21.8	165.9
	ds	36.8	271.4	23.5	311.9
Retracing errors	DS	11.6	37.7	4.1	16.5
	dS	11.2	67.7	3.9	10.7
	Ds	10.8	40.2	2.9	11.3
	ds	6.9	24.3	3.6	25.4
Time (sec.)	DS	801.2	124,425.6	489.4	15,229.9
	dS	1,055.0	989,547.7	659.1	207,055.0
	Ds	761.3	174,011.8	457.8	49,743.3
	ds	537.7	24,634.4	435.7	26,333.6

It is apparent from Table 3 that, in some cases, a marked difference exists between the group means which is attributable to the presence or absence of E.C.S. in the group treatment. Thus, for forward-going errors during the treatment phase, the group means may be arranged as follows:

	Drug (D)	No drug (d)	Total
Shock (S)	53.2	51.7	104.9
No shock (s)	40.6	36.8	77.4
Total	93.8	88.5	182.3

The analyses of variance of the data, in each case arranged in a two-way classification like the above, confirm this impression. Table 4 summarizes the results from four of the six tables of variance.

Table 4. *Results of analyses of variance*

Source of variance	Degrees of freedom	Significance of <i>F</i> -ratios			
		Forward-going errors		Log time	
		Treatment phase	Post-treatment phase	Treatment phase	Post-treatment phase
Epanutin	1	n.s.	n.s.	n.s.	n.s.
E.C.S.	1	$P < 0.001$	$0.01 > P > 0.001$	$0.05 > P > 0.01$	$P = 0.01$
Interaction	1	n.s.	n.s.	n.s.	n.s.
Within groups	64	—	—	—	—

None of the *F*-ratios for retracing errors reached the 5% level of significance; they have, therefore, been omitted from Table 4. It should be noted that none of the interactions is significantly large, thus permitting the use of the within-groups variance estimates in computing the *F*-ratios. In order to facilitate the analyses, equal numbers in each group were obtained by randomly discarding the data for one subject in each of the DS and dS groups (see Table 1), thus leaving each of the four groups with a total of 17. This was done before the values shown in Table 3 were computed.

No test of the normality of distribution of these data was made, though before each analysis of variance was attempted, Bartlett's test of homogeneity of variance (21) was applied. With two exceptions, the resulting χ^2 did not reach significance at the 5% level, i.e. such inhomogeneity as existed was within the limits to be expected by chance. The exceptions were the time scores for both treatment and post-treatment phases, the corrected χ^2 's for which were significant beyond the 0.1% level. A logarithmic transformation of the total time score for each subject was therefore made and the Bartlett's test recomputed. The χ^2 for the post-treatment phase no longer reached the 5% level of significance, but the transformation had not similarly rendered the time scores for the treatment phase satisfactorily homogeneous, for the value of χ^2 still reached the 2% level. However, the analysis of variance was computed as the assumption of homogeneity was not grossly violated.

The results of the statistical analysis clearly indicate that the shock groups are significantly inferior to the non-shock groups with respect to forward-going errors and to time, during both the treatment and post-treatment phases, but not with respect to retracing errors during either phase. Further, the differences between the groups receiving epanutin and those not receiving it nowhere reach significance, from which it follows that the basic null hypothesis which this experimental design tests (i.e. that there is no difference in the efficiency of learning of a group of rats subjected to E.C.S. and another group medicated with epanutin before E.C.S.) must be retained. It also follows that the injection of the drug did not have a significant effect on the maze performance of the rats not subjected to E.C.S. It may therefore be concluded from this study that

- (a) E.C.S. markedly retards the learning of a difficult water maze.
- (b) Epanutin in the dosage used does not protect rats from the deleterious effects of E.C.S. on learning.
- (c) Epanutin in the dosage used does not retard the learning of the maze in the absence of E.C.S.

IV. DISCUSSION

The main finding of the present experiment—that epanutin fails to protect rats from the deleterious effects of E.C.S. on maze learning—confirms the results obtained by Thomas & Stone (24), and this conclusion gains particular weight thereby. It would seem that one may further conclude from such findings that the disturbing effects of E.C.S. are not dependent on the occurrence of the overt aspect of the convulsion as such. Here we have a situation in which the tonic-clonic, grand mal type of convulsions were effectively prevented, yet in which the characteristic performance decrements following E.C.S. remained.

The absence of protection afforded by epanutin contrasts with the findings of the studies in which a general anaesthetic was used as an anticonvulsant. An explanation is probably to be sought in differences in the pharmacological action of the drugs used.

Of the hypotheses put forward to explain the decrement in performance following E.C.S., the physiological stress theory of McGinnies (11, 12) is the easiest to dispose of as an explanation of the present findings. Essentially, McGinnies suggests that E.C.S. produces a generalized enfeeblement or indifference in the shocked animals which is reflected in their poor performance. With Hayes (9), it may be assumed that such enfeeblement will affect their time scores, but not their error scores. In order to discover if the time scores of the various groups during the treatment phase differed more than could be attributed to the influence of the differences in error scores, a simple analysis of variance of the log-time scores after covariance adjustment had been made for total errors was computed (13). Total error scores were used as it was believed that these were more likely to be highly correlated with time scores than forward-going errors alone. The detailed results are given by Broadhurst (4) and show that after covariance adjustment for errors, the time scores no longer differ significantly. Granted the initial assumption, then it is believed that this finding provides an argument against the enfeeblement hypothesis.

The fear hypothesis, propounded by some authors (e.g. (22)) to account for the decrement in maze performance attributable to E.C.S., cannot be wholly discounted as a possible explanation of the poor showing of the DS group in this experiment, whatever mechanism is postulated to account for that of the dS group. It may be argued that the demonstrations by Duncan (6), by Braun, Russell & Patton (3), and by Siegel & Siegel (20) that a series of shocks to the hindlegs does not affect maze or runway performance, dispose of the fear hypothesis. But these results relate to tests of retention; it has not been shown that such a somatic shock is without effect on concurrent learning.

Unfortunately, no systematic observations of affective behaviour, which might bear on this point, were undertaken in this experiment. However, incidental observation of cage behaviour suggests that there were no striking differences between any of the groups in this respect. All rats appeared to resist the injection procedure with equal vigour. The response to the E.C.S. procedure was characteristically different, however, for the members of both of the shock groups displayed overt emotionality with 'freezing' and frequent urination and defaecation before the shock was delivered. The non-shock groups when given pseudo-shock persisted in the usual exploratory behaviour characteristic of the rat. Thus the authors have the impression that the DS and the dS groups were not distinguishable on the basis of emotionality.

A relatively simple experiment can be devised to decide the issue. A 2×2 design, as

used in the present experiment, would meet the case. As here, one independent variable would be the presence or absence of E.C.S.; the other, the presence or absence of an equivalent somatic shock. Learning a maze under water motivation would test the effects of the different group treatments.

Even if fear cannot be discounted as a possible cause of the inferior performance of our DS group, then neither can the physiological effects of the non-convulsive cerebral shock. The results obtained by Hayes⁽⁹⁾ from his 'petit mal confusion group', which were given such treatment, suggest that fear did not cause the observed retardation in their concurrent learning. Bernberg⁽¹⁾ showed that lengthy electronarcotic treatment of young rats significantly impedes their later learning, though the results of Braun, Pierce & Patton⁽²⁾ demonstrate that sub-convulsive electric shocks do not. But immature rats were used in both of the last-mentioned researches, which makes the results obtained not strictly comparable with our own.

It would seem, therefore, that both fear and physiological effects dependent on the passage of current probably contributed to the retardation of learning displayed by our DS group.

V. SUMMARY

1. After three daily trials on a difficult water maze, seventy rats were assigned to four groups which received all possible combinations of two treatments: (a) medication with epanutin, and (b) electroconvulsive shock. The next twelve trials were made under the influence of these treatments. After the treatments ceased, another twelve trials were allowed.

2. Analysis of variance showed that the groups receiving E.C.S. were significantly inferior to those not receiving E.C.S. in respect of forward-going errors and time scores, both during the treatment and post-treatment phases. The presence or absence of epanutin in the group treatment led to no significant differences.

3. Previous findings that epanutin is ineffective in offsetting the retarding effect of E.C.S. on maze learning are thus confirmed. Possible explanatory hypotheses are discussed.

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THE VIGOTSKY TEST AS A MEASURE OF INTELLIGENCE¹

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I. INTRODUCTION

The writers' aim in this paper is to describe the standardization of a method of application of the test variously known as the Vigotsky and the Hanfmann-Kasanin Test. The process studied is that of concept formation, and the method of testing has its origin in the work of Ach⁽¹⁾. Although the earliest work was in the study of normal thought processes, interest was rapidly diverted to disorders of conceptual thinking in schizophrenia, a trend which is reflected in most of the published literature. It is no part of the writers' intention to question the validity of the method as a clinical instrument, and in fact it is recognized that there are aspects of an individual performance on such a test which cannot be expressed in simple numerical terms. Nevertheless, since the situation is one which clearly involves thought processes entirely comparable with those underlying the solution of the problems which form the material of recognized tests of intelligence, it has been thought worth while to develop, and to report here, a method of scoring whereby individual performances may be assessed for effectiveness.

The method of administration used was evolved by Semeonoff as part of a testing programme carried out in connexion with a war-time 'Special Service' selection procedure which it is hoped to describe more fully elsewhere. Most of the candidates were not English speaking; for this and other reasons it was thought desirable to make use of tests of the 'performance' type, including, if possible, tests based on principles other than those of the form-board or of design construction. It was decided, therefore, to try out the test referred to here as the Vigotsky Test. This name has been preferred to the more commonly used 'Hanfmann-Kasanin', partly because the latter name has come to be associated with the clinical application of the test, and partly because of slight differences in detail of material (as well as modifications in administration) which we believe to be peculiar to our own work.

II. TEST MATERIAL AND METHOD

The test material consists of twenty-two wooden blocks, varying in colour, shape of cross-section, thickness and area of cross-section (which will be referred to as 'size'). The problem is to divide or classify the blocks into four groups in such a way that all the blocks in any one group have qualities in common which warrant their being described as a group and at the same time distinguish them from the blocks in any of the other groups. Five different colours are represented, six different shapes, two thicknesses (in the ratio of approximately 2:1) and two sizes (in the same approximate ratio). The

¹ Based in part on a paper read by the senior author at the Liverpool meeting of the British Psychological Society, 1951, and on a thesis presented by the junior author for the degree of B.Sc. with Honours in Psychology, June 1950.

orthodox or 'correct' solution depends on a combination of the two variables of size and thickness, thus producing groups which may be described as, respectively, 'large and thick', 'small and thick', 'large and thin', and 'small and thin'. Other solutions, varying in the degree to which they may be described as logically defensible, are possible; recognition of this fact underlies the clinical use of the test, and indeed provides a basis for administering it, since the problem is not one which lends itself to quick solution by persons of average ability.

Each group is identified by means of a number, which is marked on the underside of every block belonging to that group. The blocks are presented to the subject in random arrangement, with no identification marks showing. The nature of the problem is explained, and the subject told that it is unlikely that he will hit upon the correct solution at the first attempt. He should therefore tentatively offer any solution which occurs to him, and if this is not the correct solution help will be given in the following manner. One sample piece is turned up, its identifying number being exposed, and it is pointed out that when the correct classification is achieved, all the blocks in any one group will bear the same identifying number. Each time, therefore, that an incorrect solution is offered, it will be possible to give the subject a 'clue' by turning up one more block. Further solutions will then depend on recognition of the fact that differently numbered blocks must belong to different groups. Observation and analysis of differences (and later, perhaps, similarities) between exposed blocks will, of course, narrow this field of possibilities and lead towards the correct solution.

Further details of administration, together with specifications of the blocks at present in use, will, it is hoped, be published shortly. Meantime a paper of Instructions may be had on application to the senior author.

III. SCORING FOR GRADE OF SOLUTION

Hanfmann and Kasanin's original scoring method⁽³⁾ was to add to the time taken to achieve the correct solution, measured in minutes, four points for each 'clue' received, i.e. each block exposed after an incorrect solution had been offered. Thus low numerical scores on this system represent good performance. No statistical basis for this weighting is quoted but, as will be seen from the following discussion, the weight of four for each clue is fairly satisfactory. Experience with the test, however, suggested that certain subjects who solved the problem on a more or less perceptual basis, without adequate understanding of the principles involved, received too favourable scores. In order to compensate for such errors in estimation of quality of performance, a further variable 'grade of solution' was introduced. On completion of the task, the subject is asked to explain the principle on which his final (i.e. the correct) solution is based. Six grades of solution were distinguished, corresponding to successive steps away from the level of abstraction represented by an ideal solution of the problem. The six grades are as follows:

Grade I. Any solution which takes account of the fact that two qualities have to be combined. In addition to the combination of thickness and size, other combinations (e.g. of thickness or size with volume or even weight) are occasionally offered, and may be admitted as satisfactory.

Grade II. Correct solution, as above, elicited with a minimum of prompting; or a solution in which one variable is definitely regarded as subsidiary to the other.

Grade III. More or less intuitive apprehension in terms of a single element, usually with some sort of concrete reference, e.g. members of one group considered as ‘halves’ of those of another.

Grade IV. Any of the above, but with persistent mention of irrelevant elements, e.g. each colour or shape represented ‘as far as possible’ in each group, i.e. some adherence to the principle of distribution as opposed to classification.

Grade V. Failure to achieve any sort of coherent explanation, even with prompting and encouragement. In such a case the blocks should be mixed, with the identifying marks concealed, and the subject invited to reproduce the final solution. Grade V is credited only if this is correctly done.

Grade VI. As above, but subject fails to re-group the blocks correctly. Properly speaking, this performance should be regarded as a failure, but since the test was completed, albeit without insight, it seems reasonable to credit a ‘solution’.

Assessment of grade of solution on the above basis should ideally, of course, be unambiguous, and any difficulty that may be encountered usually arises from differences in the amount of encouragement or prompting which individual testers may consider to be desirable and legitimate at this as well as at earlier stages in the administration. This point is more fully treated in § V below. It must also be realized that while fluency in verbalization will undoubtedly have its effect on the apparent satisfactoriness of the explanation, what one is really looking for is evidence of understanding of the conceptual framework of the problem and its solution. The introduction of an essentially verbal element into a ‘performance’ situation was a step which was regretfully taken, but the added diagnostic value it gave to the test procedure seemed unquestionable.

For the purpose of incorporating grade of solution in a composite test score there appeared to be no practicable alternative to assuming an equal quantitative difference in ‘satisfactoriness’ between the various grades. This is of course a highly questionable assumption, and, as Laird (4) points out, the high proportion of grade I solutions obtained (see Table 1) indicates that this variable is not discriminating usefully at the topmost levels. On the other hand, there seems to be little justification for allowing added credit for precision of verbalization, nor in any case does there appear to be any reason to suppose that the grade of solution should be a normally distributed component of the score.

Table 1. *Grade of solution distributions*

Grade	Sample			Total (<i>n</i> = 378)
	I (<i>n</i> = 206)	II (<i>n</i> = 72)	III (<i>n</i> = 100)	
I	114	43	39	196
II	49	10	26	85
III	25	8	13	46
IV	7	2	4	13
V	7	8	14	29
VI	4	1	4	9

IV. STANDARDIZATION

(i) *The original sample*

As already mentioned, the method of scoring just described was evolved empirically during the application of a selection procedure. This testing was carried out during 1943–4, either by Semeonoff personally, or under his direct supervision. In all, about

1000 candidates were examined. In the early stages of the investigation all candidates were given Vigotsky, but experience progressively showed that the problem was rather a formidable one for the lower intelligence levels and, as alternative tests became available, the number of candidates to whom the Vigotsky test was given decreased, and the sample became less representative of the total candidate population. For this reason it was difficult to scale the scores in relation to general population standards, or even to those of the candidate population. Consequently neither this group of cases (sample I, numbering 206), nor a further sample of 72 cases (sample II) collected in 1945 at No. 14 War Office Selection Board, has been included in the standardization carried out by Laird in 1949-50 (sample III). Distributions of grades of solution for these two groups, are, however, included in Table 1. In examining this Table it should be borne in mind that the mean of sample I for intelligence tests proper fell at about the seventieth percentile of the general population, while that of sample II (Officer candidates) would probably fall at about the eighty-fifth percentile of the general population.

Analysis of test results on the original sample of 206 cases showed approximately equal inter-correlations of the three components of the score (time, clues and grade of solution), and also almost equal correlations with scores on Raven's Progressive Matrices. Weights in inverse proportion to the respective standard deviations were therefore applied, and these, to the nearest whole numbers, yielded the formula

$$\text{score} = \text{time} + 3 (\text{clues}) + 11 (\text{grade} - 1).$$

In applying this formula, time was measured to the nearest minute, clues received did not include the first or 'sample' clue, and grade of solution contained the '-1' element in order to reduce the score of a grade I solution to zero, and thus make zero the theoretical best score (i.e. time less than $\frac{1}{2}$ min. no clues, perfect explanation).

For reasons inherent in the conditions of the investigation little importance is attached to these weights. Table 2 shows the tentative scores which were assigned to certain percentile points for a hypothetical general population.

Table 2. *Percentile norms for sample I*

General population percentile	95	75	50	25	5
Semeonoff norms	18	40	60	72	113
Laird norms	25	57	76	99	145

Scoring Laird's group (see following subsection) on the same system yields the figures in the third line of the above table, and it will be seen that the standards set by the original standardization appear somewhat too stringent.

(ii) *Laird's re-standardization*

The re-standardization which forms the main subject of this paper was carried out by Laird during 1949-50. The group studied consisted of 107 subjects between the ages of 16 and 29. About thirty additional cases outside this age range were excluded, since it was believed that scores might be expected to rise with age below the 15-16 year level, and since there seemed to be evidence (see, for example, Foulds & Raven(2)) that capacity to think in terms relevant to the test begins to decrease round about the age of 30.

An attempt was made to assemble a sample as nearly as possible representative of the general population. Laird's unpublished work contains full details of age and occupation of the subjects: Table 3 summarizes the occupational distribution.

Table 3. *Distribution of subjects according to occupation*

Professional and academic	18
Semi-professional and managerial	10
Trained clerical and skilled trades	25
Semi-skilled occupations	33
Trades requiring some but little training	14
Unskilled work	7

Testing was in all cases voluntary, and no payment was made.

Of the 107 cases tested, seven failed to achieve a solution in eighty minutes; these performances were recorded as ‘unfinished’. They were, however, included in the distributions of time and clues scores on which calculations were based. Since no solution was reached, they could not be included in the grade scores distribution; the latter is thus based on 100 cases.

The distributions of scores on the three components yielded the following means and standard deviations: time, 17.6 ± 0.81 ; clues, 3.5 ± 0.16 ; grade, 1.6 ± 0.07 . These lead to simple inverse weightings of 1, 5 and 11 respectively. The main difference from the earlier standardization will thus be seen to be in respect of clues scores, and incidentally suggests that the original change from the Hanfmann-Kasanin system was in the wrong direction.

Need for a further adjustment, however, at least in theory, arose from a consideration of the correlations of the various components with the Progressive Matrices criterion and with each other. From the correlation matrix shown in Table 4 regression coefficients of

Table 4. *Correlation matrix of score components*

	Time	Clues	Grade
Time	1	0.42	0.11
Clues	0.42	1	0.21
Grade	0.11	0.21	1
Progressive Matrices	0.44	0.46	0.28

0.29 (time), 0.30 (clues) and 0.18 (grade) were calculated. Multiplying these by the s.d. weights already assessed, and adjusting again to nearest whole numbers, with time score as unity, gives weights of: time 1, clues 5, and grade 7. These are, accordingly, the weights recommended for use in computing full Vigotsky scores.

Provisional percentile norms based on the Laird sample are as shown in Table 5.

Table 5. *Provisional percentile norms*

Percentile of general population	95	90	75	50	25	10	5
Score	36	47	64	87	103	131	150

V. VALIDITY AND RELIABILITY

Correlations of full Vigotsky scores with other tests have consistently yielded positive coefficients. The Laird sample gave a correlation with Progressive Matrices of 0.54 ± 0.05 . Semeonoff’s earlier work yielded correlations (with a selected group) of between 0.4 and 0.5 with both Progressive Matrices (all versions) and ‘Reasoning’ (Army S.P. 45), and of 0.57 ± 0.04 with Kohs’s Blocks (Trist-Misselbrook version). No attempt has been made to assess *g* or other factor loadings, but it would seem to be clear that while Vigotsky

cannot be regarded as a suitable alternative to commonly accepted intelligence tests, it is as possibilities as an instrument for the study of at least the qualitative aspects of intelligent performance. Many circumstances, on the other hand, inherent in the nature of the test problem, make it impossible to place much reliance on a Vigotsky score. Similar considerations make it extremely difficult to demonstrate reliability in the statistical sense. Split-half and re-test reliability are equally impracticable, for obvious reasons, and even re-test with 'equivalent' or other alternative material is almost out of the question, since the basic principle of the solution, once understood, is not readily forgotten. Both writers are of the opinion that if a test which purports to be a quantitative measuring instrument cannot be shown to be reliable there can be little justification for its continued use *in that particular role*. And into this category the Vigotsky test would seem to fall.

On the other hand, Laird (4) reports some preliminary work with an 'alternative' test, the outstanding result of which was that several of the behaviour patterns which had characterized an individual's Vigotsky performance proper were again revealed in the re-test'. This applied no less to features which could be regarded as predominantly cognitive—e.g. indications of the adoption of hypotheses of the types described by Lapoport (7) as 'rigid' or 'flexible', or evidence of 'conceptual' and 'perceptual' thinking (Hanfmann & Kasanin (3))—than to those which are conventionally more closely associated with temperament: indications of irritability, resignation, impulsiveness, deliberation and the like. This, however, is not to be regarded as in any way compensating for the admitted absence of statistical reliability, but it helps to call attention to the possibility of consistency in an individual's adjustment to test situations comparable with that furnished by the Vigotsky test. An adequate scoring system may then, as far as it goes, be regarded as providing a suitable framework within which a total performance is viewed. So long as the end-product (which traditional intelligence testing appears to demand, and which emerges in the form of a composite score, mental age, I.Q. or percentile rating) is not allowed to obscure the *processes* by which such an end-product has been achieved, there can be no objection to making use of a composite score as a kind of summing up in the last instance.

As regards inter-examiner consistency, it must be admitted that the technique of administration of the Vigotsky test is a skill which is not easy to acquire. Rules for administration and scoring are laid down in the Instructions, but while the latter process can be carried out at leisure without fear of going wrong, the beginner may find himself at a loss, or at any rate, hesitant, on some point of administration. Thus, for example, one may not be able to decide quickly and confidently which is the correct clue to furnish at a given point in the procedure. An error or variation here may alter the entire course of the subject's subsequent performance. Much, again, depends on the examiner's patience, and on his judgement as to what is an appropriate amount of prompting or encouragement.

One expedient which should definitely not be adopted is the convention that a clue may be 'forced' on the subject after five minutes inactivity, or any other such arbitrary period. To do so obviously makes clues received far too direct a function of time taken, and undermines the whole scoring system. On the other hand, the student who is learning to administer Vigotsky may find it convenient to set a period to his own silences, and if the prompting given is sufficiently non-committal, the subject's performance will not be

seriously affected. It is in relation to such points as these, however, that inter-examiner reliability may falter.

A specific problem in this connexion, and one which really deserves extended separate treatment, is how to deal with 'defensible alternatives'. By this is meant occasions when the subject offers a logically unexceptionable solution other than the orthodox or expected one. If the subject is able, without discouragement, to accept the convention that such a solution is 'wrong', no difficulty need arise. Otherwise it is probably advisable to abandon the attempt to score the test, and to treat it wholly qualitatively, and it might be argued that even in the former case the score may not adequately reflect the subject's success in dealing with the problem as formulated.

Further consideration of these points is reserved for another occasion.

VI. FURTHER MODIFICATIONS

Whatever attitude is taken to the general problem of scoring, it is hoped that those who use the test will continue to record detailed scores, particularly in respect of grade of solution. To refer again to the figures in Table 1, it will be seen that in all groups studied a very small proportion of grade IV solutions was obtained. It seems likely that for practical purposes it would be justifiable to combine grades III and IV as a single grade and to renumber grades V and VI accordingly. Such a step would simplify the task of assessing grade of solution without seriously detracting from the validity of the composite weighted score. Laird showed that reducing the six grades to five in this way and retaining a weighting factor of 7 left the multiple correlation coefficients relatively unaffected. It is now suggested, however, that if Grade IV solutions continue to be few it might be advantageous to accompany the reduction to five grades by an increase of the grade weighting factor to 10. This would make for much greater convenience in scoring, and at the same time 'improve' the poorer grade scores to a lesser extent. Incidentally, the application of regression weights and consequent reduction of grade weight from 11 to 7 (see § IV (ii) above) affected the multiple correlation coefficient only in the second decimal place. This difference was found to be significant, but only at the 2% level, and a realignment of the scores according to the proposed modification would be unlikely to have any marked effect, except as it would affect a small proportion of individual scores.

Comparison of scores obtained by the different methods will be facilitated by reference to Table 6.

Table 6. *Grade of solution scores calculated by different methods*

Grade	Semeonoff and Laird (without regression weights)	Laird (4)	Laird (condensed scale)	Proposed (condensed scale)
I	0	0	0	0
II	11	7	7	10
III	22	14	14	20
IV	33	21		
V	44	28	21	30
VI	55	35	28	40

VII. DISCUSSION

In conclusion, we wish to repeat that our apparent preoccupation here with the scoring of the Vigotsky test as a measure of intelligence in no way reflects our view of what we consider to be the principal value of the test—namely, its ability to throw some light on

the stage-by-stage processes, and not only on the end-products, of problem solving. We have already (in § V) indicated some of the behaviour patterns which emerged from continuous detailed recording of Vigotsky performances. The test protocols⁽⁴⁾ threw up numerous others—distinctive ways of absorbing fresh clue information, particular ways of reacting to blockages and prolonged failure, individual differences in range of alternative hypotheses, varying tendencies to discard or to retain faulty hypotheses, amount and nature of accompanying verbalization, etc. Essentially, the practical usefulness of a qualitative analysis must depend on how adequately and objectively such test responses can be described and thus rendered recognizable in any Vigotsky performance. Some progress in this direction will be reported in a separate paper⁽⁵⁾. Here we would merely emphasize one rather obvious implication of such an approach. Many of our subjects who revealed markedly different or even contrasting modes of tackling and solving the test problem obtained equivalent Vigotsky scores; and many of these also obtained nearly equal Matrices scores. If one is interested not only in *how much* 'intelligence', 'intellectual capacity', etc. an individual may be said to have, but also in *how* he uses what he has, how he applies himself and adjusts himself to a problem, surely one will set unnecessary limits to the field available for study if one clings to traditional test methods which, by their very construction, make it extremely difficult, if not impossible, to find out anything definite about a subject's test performance until he has actually completed it?

The usual objection made at this stage is that one is no longer talking purely in terms of cognitive processes, but rather in terms of individual personality characteristics, more properly the subject-matter of special procedures designed for their assessment. To some extent this is true. There are, for example, other and more satisfactory objective methods for investigating singly most of the temperamental and general behavioural traits that characterize Vigotsky performance. Or, again, a 'global' picture can be obtained from the use of such techniques as Rorschach or T.A.T. But few of these special methods allow the subject to be observed in an essentially problem-solving situation, and hence the light they throw on the cognitive process proper is negligible. Our main point is, therefore, that between the field of personality assessment, in the broadest sense, and that of traditional intelligence testing, lies a gap which requires to be filled by the study of the pre-solution stages of cognitive thinking. Perhaps the richest field for such study lies in 'practical situation' testing, as investigated, for example by Maier⁽⁶⁾, but our work with the Vigotsky test encourages the view that some contribution is possible in the more orthodox test situation. We are not suggesting that the Vigotsky test is ideally suited for the task, but merely that research in this field of cognitive processes, with suitably constructed techniques, is long overdue.

VIII. SUMMARY

A preliminary investigation, carried out for selection purposes under war-time conditions, suggested that the Vigotsky (or Hanfmann-Kasanin) test might usefully be employed as an intelligence measure, in addition to its normal use as a means of studying the nature of cognitive processes. A scoring system was evolved in which credit was given for adequacy of explanation of the solution obtained, in addition to score components based on time taken and number of clues received.

The scoring system was re-standardized on a group of 107 subjects representative of

the general population, and further minor modifications in scoring are now proposed. The validity and reliability of the test as an intelligence measure are discussed. While it is claimed that a numerical score is not without meaning, it is admitted that it is virtually impossible to demonstrate reliability. The test situation, however, affords ample opportunity for the study of the processes rather than the products of conceptual thinking, and as such it offers valuable or even indispensable supplementary material to the results of orthodox intelligence testing.

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THE WORD CONNEXION LIST AS A DIAGNOSTIC TEST: NORMS AND VALIDATION

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I. *Introduction* (p. 103). II. *Norms and validation* (pp. 103-108). III. *Discussion* (pp. 108-109). IV. *Summary and conclusions* (pp. 109-110). *Appendix* (p. 111). *References* (p. 112).

I. INTRODUCTION

The Word Connexion List is a controlled association test in which each item consists of a stimulus word and two possible response words, one of which is an 'abnormal' response, the other a 'normal' response, e.g. SINK: wash, drown. The subject is scored on the number of 'abnormal' choices he underlines.

The fifty items forming the present test were selected from a 200-item test designed by Malamud (1946). Malamud's W.C.L. was administered to large groups of neurotic and normal persons, and an extensive item analysis was carried out to determine the items which best differentiated these groups (Crown, 1947).

The present paper is concerned with establishing norms for the fifty-item W.C.L., with its validation and with the discussion of some points of theoretical and practical interest which have arisen in the course of this work.

II. NORMS AND VALIDATION

All psychologists in this country who had previously shown interest in the possibility of using the test, after its written and verbal presentation by the writer (Crown, 1947), were asked whether they had made use of the test. If so, they were asked if they would send to the writer a description of the group to whom they had administered the test and for the mean score and standard deviation of the group. In addition, original data from large service and other groups were analysed by the writer, and further data were taken from large-scale researches being carried out in this Department. These data are represented in Fig. 1. Full descriptions of the groups are given in the annotation to the figure.

Fig. 1 shows the mean scores and standard deviations of scores on the W.C.L. for twenty-eight normal and abnormal groups, containing altogether 3345 persons, of whom 1840 are normal persons and 505 abnormal. The differentiation between 'normal' and 'abnormal' is on the following grounds: The 'abnormal' persons have been diagnosed by psychiatrists as psychiatrically abnormal, have been institutionalized for anti-social behaviour or suffer from physical diseases which have been considered by some authorities to have an appreciable emotional component. The 'normal' groups, although they may contain past members of all the 'abnormal' groups and present members of the first category and the last, are persons who have been taken from a variety of occupations, social strata and intelligence levels in ordinary life. They form, in fact, a more heterogeneous group than is usually tested in the standardization of a psychological test.

Although some of the groups are small, the main impression from Fig. 1 is that the scores of the abnormal groups tend to be higher than the scores of the normal groups.

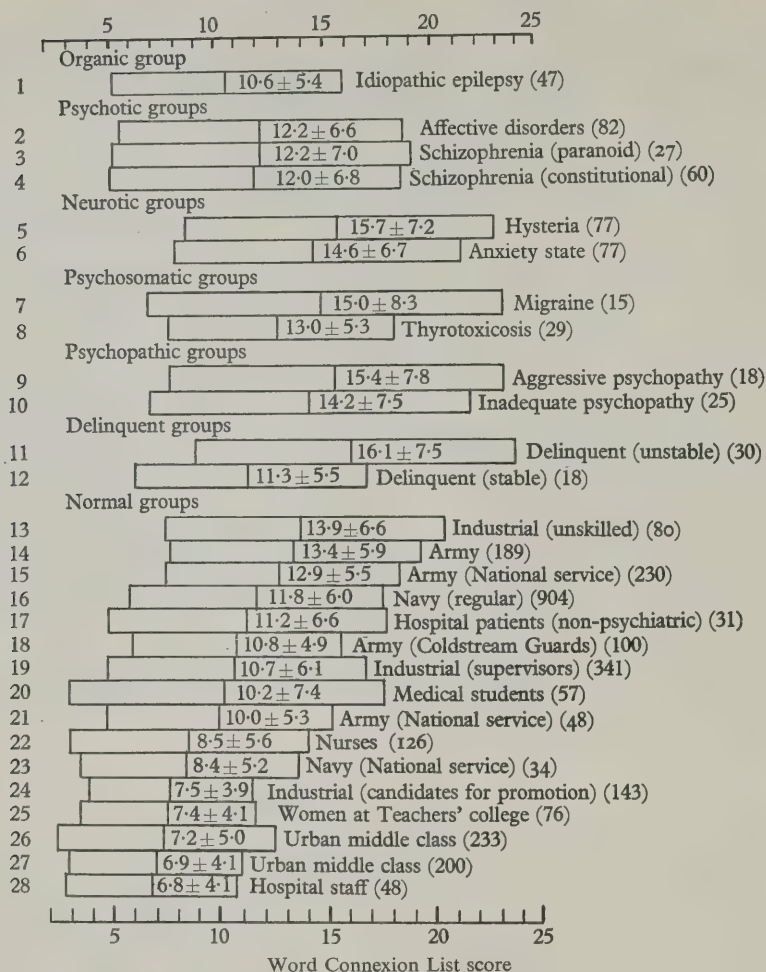


Fig. 1. Word Connexion List: mean scores and standard deviations of twenty-eight normal and abnormal groups.

Annotation to Fig. 1. Description and source of the groups tested¹

Group reference number	Source	Description
<i>Organic group</i>		
1	J. W. Lovett Doust (unpublished) M. D. Eysenck (1950)	Idiopathic epilepsy
<i>Psychotic groups</i>		
2	J. W. Lovett Doust (unpublished) H. J. Eysenck (1951a)	Affective disorders: depression. Including cases of agitated and tension depression; manic depression-depressive type; involuntional melancholia and puerperal depression
3	J. W. Lovett Doust (unpublished) H. J. Eysenck (1951a)	Paranoid schizophrenia, paraphrenia; acute schizophrenic reaction
4	J. W. Lovett Doust (unpublished) H. J. Eysenck (1951a)	Constitutional schizophrenia: including cases of dementia praecox simplex; catatonia; hebephrenia
<i>Neurotic groups</i>		
5	J. W. Lovett Doust (unpublished) H. J. Eysenck (1952) B. Clarke (unpublished)	Hysteria: including examples both of conversion hysteria and of hysterical personality

¹ Several of the studies listed in the annotation to Fig. 1 consist of unpublished information. If any reader would like further information on these groups the author would willingly supply it.

Annotation to Fig. 1. (continued)

Group reference number	Source	Description
<i>Psychosomatic groups</i>		
6	J. W. Lovett Doust (unpublished) H. J. Eysenck (1952) B. Clarke (unpublished)	Anxiety state: including examples of acute and chronic anxiety states, phobic anxiety states and obsessional neurosis
7	J. W. Lovett Doust (unpublished)	Migraine: a group of outpatients, suffering from migraine and without other psychiatric symptomatology
8	J. L. Standen (1950)	Patients suffering from thyrotoxicosis. These had been tested with radioactive iodine, had received medical or surgical treatment or both, and were in various stages of recovery
<i>Psychopathic groups</i>		
9	J. W. Lovett Doust (unpublished) H. J. Eysenck (1952)	Psychopathic personality: mainly in-patients. Including predominantly 'aggressive' cases of psychopathy
10	J. W. Lovett Doust (unpublished) H. J. Eysenck (1952)	Psychopathic personality: mainly in-patients. Including predominantly 'inadequate' cases of psychopathy
<i>Delinquent groups</i>		
11	C. Gore (unpublished)	Male delinquents in prison, regarded clinically as exhibiting definite psychiatric abnormality: long-standing personality deviation, e.g. psychopathy, alcoholism, persistent sexual maladjustment; neurotic symptoms of varying severity and type; and symptoms suggestive of psychotic reactions
12	C. Gore (unpublished)	Male delinquents in prison, regarded clinically as showing no definite psychiatric abnormality, nor giving a history indicative of instability in the past
<i>Normal groups</i>		
13	A. Heron (1951)	Group of male 'casters' of battery grids at a large accumulator factory. They use hand-operated moulds, and the task can be regarded as unskilled or barely semi-skilled
14	H. J. Eysenck (1952)	Army (Royal Artillery) personnel
15	I. R. Haldane (unpublished)	Army (Royal Artillery, National Service) personnel
16	Admiralty (unpublished)	Navy (Special Service, 7 years, and Continuous Service, 12 years) personnel
17	J. L. Standen (1950)	Patients for a control group selected from the wards of a non-psychiatric hospital, the main criteria being availability and freedom from psychosomatic symptoms
18	H. J. Eysenck (1951a)	Army (2nd Battalion, Coldstream Guards) personnel
19	Castle & Garforth (1951)	Industrial sample, foremen and candidates for supervisors' vacancies
20	Petrie (1948)	Medical students, both sexes, St George's Hospital Medical School
21	B. Clark (unpublished)	Army (Dental Corps, National Service) personnel
22	Petrie & Powell (1950)	Nurses, who had been in training, at St George's Hospital for over 6 months
23	Admiralty (unpublished)	Navy (National Service) personnel
24	J. Munro Fraser (unpublished)	Industrial sample: both sexes, candidates tested during selection procedures for promotion
25	Loos (1951)	Student teachers
26	H. J. Eysenck (1951b)	Urban middle class sample
27	Crown (1950)	Urban middle class sample
28	J. W. Lovett Doust (unpublished)	Hospital staff: these include psychiatrists, psychologists, nurses, technical and clerical staff and a few others, e.g. non-psychiatric doctors, a biochemist, etc.

There is, however, considerable overlap among the groups; two particular qualifications must be made. First, there are considerable sampling variations among the normal groups; secondly, psychotic groups do not tend to score highly on the test. These points must be discussed further.

The means of the scores of the normal groups vary by as much as 7.1 points, the most 'normal' of these groups having a mean of 6.8 and the most 'abnormal' a mean of 13.9. In part, these observed variations in mean score on the W.C.L. among normal groups may be due to differences in intelligence between the groups. The precise relation between score on the W.C.L. and intelligence is as yet unknown, but the correlations reported in Table 1 would suggest that the relation tends to be negative and low, and that it varies from sample to sample. The relation may be curvilinear. P. F. C. Castle (personal communication), for example, reports that with the industrial group he was investigating (Castle & Garforth, 1951), the relation between the W.C.L. and intelligence was significantly curvilinear ($r = -0.23$, $\eta = 0.40$). The precise relation between the W.C.L. and intelligence and the underlying causes for the curvilinearity, if this should be confirmed, are not of direct relevance in this discussion. Suffice it to suggest here that variations in intelligence among the groups investigated may, at least in part, be responsible for the observed variations in mean score on the W.C.L. among these groups.¹

Table 1. *Word Connexion List: correlation with tests of intelligence*

Group reference number (cf. anno- tation to Fig. 1)	Description and source	N	Intelligence test(s)	r
13	Industrial (Heron, 1951)	80	Dominoes	-0.41
14	Army (Eysenck, 1952)	92	Progressive Matrices SG.	+0.04
16	Navy (Admiralty, unpublished)	904	S.P.I. (abstractions)	-0.33
19	Industrial (Castle & Garforth, 1951)	341	N.I.I.P., 2 x G.T. 72 (non-verbal, perceptual) + G.T. 91 (verbal)	-0.23
20	Medical Students (Petrie, 1948)	57	Sentence completion (Thorndike)	-0.25
22	Nurses (Petrie & Powell, 1950)	126	Pattern perception (Penrose)	-0.29
23	Navy (Admiralty, unpublished)	34	S.P.I. (abstractions)	-0.15
25	Student teachers (Loos, 1951)	76	P.M.A. reasoning (Thurstone)	+0.10

Nevertheless, from both the practical and the theoretical points of view, the finding that there are considerable sampling variations in neuroticism is an important one. It serves to emphasize the necessity of describing carefully the 'normal' sample tested in any particular investigation. It also underlines the necessity for caution in drawing conclusions from the results of a single research, where the design involves the comparison of the test performance of normal with abnormal groups.

These points are well illustrated in two researches, the first carried out by M. D. Eysenck (1950). In this, the W.C.L. and another written test of neuroticism, the Maudsley Medical Questionnaire, were used to test the hypothesis that a group of patients suffering from idiopathic epilepsy would differ from normals in their scores on tests of neuroticism, scoring more abnormally. The norms taken by M. D. Eysenck for comparison with her epileptic group were supplied to her by the present author, being all that was available at the time, and the hypothesis was considered confirmed. Since that time, more data on the W.C.L. have accumulated, and it would now seem that, at least on this test, the evidence confirming the hypothesis is less certain. This is not to say that the findings of M. D. Eysenck's paper have been invalidated, however, as the mean score of the epileptic group on the other questionnaire, the Maudsley Medical Questionnaire, was markedly abnormal.

¹ Since the time of writing Prof. Vernon has suggested in a personal communication that possibly an educational or socio-economic factor rather than intelligence is involved in the differences between the groups.

A second research illustrating the need for caution in interpreting the results of a single investigation is that of Gravely (1950). In this study, comparisons on the W.C.L. and other tests were made between neurotic and normal soldiers in order, among other aims, to investigate the screening efficiency of the tests. It was concluded that the screening efficiency of the tests was too low for the tests to have any practical value. In fact, it can be seen from Fig. 1 that the mean score of the normal sample from which Gravely drew her subjects for comparison with her neurotic groups (group reference number: 14) was among the two most neurotic of sixteen normal groups tested. Seen in this light, at least the future practical screening possibilities of the tests used in Gravely's battery may seem rather more hopeful.

The second qualification made to the statement that the abnormal groups tend to score more highly than the normal groups concerned the position of the psychotic samples. In Fig. 2 the combined means and standard deviation for the normal groups, the neurotic

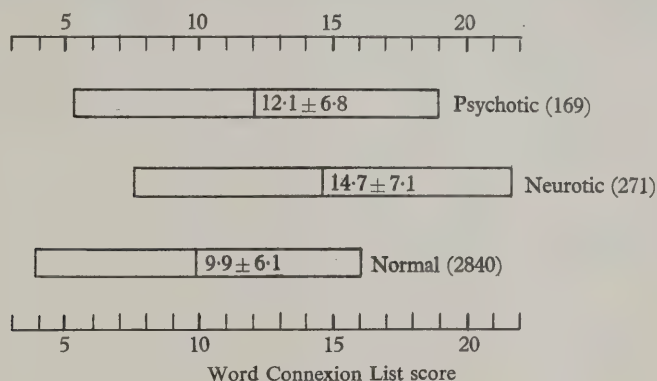


Fig. 2

groups (including the psychosomatic groups, the psychopathic groups and the delinquent-neurotic group) and the psychotic groups are represented, weighted according to the formulae given by Guilford (1936). Depressive and schizophrenic groups clearly tend to score lower on the test than the neurotic groups. This finding is an interesting and important one. Eysenck (1952) has used this, together with other findings in which neurotic and psychotic patients have performed very differently on psychological tests, to support his contention that the variation from normality of the psychotic and neurotic illnesses is along different, uncorrelated, dimensions.

Two interesting suggestions arise from the statistics in Fig. 1 concerning the psychosomatic (migraine and thyrotoxicosis) groups and the psychopathic (aggressive and inadequate) groups. These tend to score on the W.C.L. as neurotic, rather than as normal groups. As regards the psychosomatic groups, the figures give some confirmation to the suggestions from researches summarized by Saul (1944) that there may be an appreciable emotional component in these particular disorders. It is more difficult to interpret the results from the psychopathic groups. Eysenck (1952), who reports extensive psychological testing of normal, neurotic and psychopathic persons (his group of psychopaths is included in the present group), concludes of the relation between neuroticism and psychopathy that '... psychopathy is likely to have projections on the neuroticism axis but unlikely to lie on it.' Although, in the present research, we have added to the size of

Eysenck's group of psychopaths, data is available only on the W.C.L., and so his conclusion cannot be elaborated. We may conclude that persons with psychopathic personalities, as these groups are described in the annotation to Fig. 1, in addition to the other ways in which they may differ from normals, tend to be more neurotic.

III. DISCUSSION

As far as the use of the W.C.L. is concerned no 'critical score' above which a person may be considered 'abnormal' will be presented here.¹ Rather the annotations to Table 1 are detailed enough for an investigator to compare any individual score with the scores recorded for these groups. In this way he can get some idea as to how far a given score is suggestive of abnormality.

Users of tests similar to the W.C.L., Malamud (1946), for example, and Wolff (1946), have on the whole tended to emphasize their practical use; and the present W.C.L. has been used in several selection studies. Petrie (1948), for example, in an investigation into the possibility of improving the selection of medical students by the use of psychological tests, found that the W.C.L. correlated +0.43 with an index of inaccuracy (number of false answers given in all the tests expressed as a ratio of the total number of answers attempted in these tests) which she found to be correlated with success in examinations when intelligence was held constant. Petrie & Powell (1950) found the W.C.L. useful as part of a battery of tests for the selection of nurses; it was one of a group of twelve personality and intelligence tests which she showed were related to the performance of the nurses as estimated by the use of rating scales. Castle & Garforth (1951), in an exploratory investigation into the use of tests in the selection of supervisors in industry, report low, but statistically significant, correlations between the W.C.L. and selection board ratings of a group of forty-one foremen (-0.37) and 280 applicants for promotion to foreman (-0.13).²

Despite the interest of the practical studies, they do not throw much light on the crucial problem of the validation of the test as a measure of neuroticism, for neuroticism is only one of many factors which makes for success or otherwise among nurses and medical students, and in industry. Nor do the data in Figs. 1 and 2 give any precise information on this question; for nothing can be said about the screening efficiency of the test so long as the reliability of the various criteria of 'neuroticism' is unknown.

Some information on the validity of the test is given by considering the saturation of the W.C.L. with the neuroticism factor identified in Eysenck's (1952) factor analysis of a battery of twenty-eight tests selected in an effort to obtain a quantitative measure of this factor. The saturation of the W.C.L. with neuroticism is 0.301. This saturation must be considered low. The W.C.L. does, however, have projections on other factors identified in this analysis; thus it has a saturation of -0.157 on the verbal neuroticism factor, and low positive saturations on the intelligence factor and on two other factors extracted but not identified in this analysis. It seems likely, therefore, that the W.C.L. cannot in any way be considered a 'pure' measure of neuroticism; accordingly, it would not be expected

¹ In a previous article (Crown, 1947), 8 was suggested as a cutting score. In the light of the more extensive normative data this suggestion is withdrawn and instead the plan mentioned below is put forward as the most helpful for users of the test.

² As these coefficients are low, the present author checked their significance using the exact test suggested by R. A. Fisher (*Statistical Methods for Research Workers*, 10th ed. p. 193). Both coefficients were significant at the 5% level.

to correlate too highly with the neuroticism factor. The question of the validity of the test has not been settled with any certainty. It would seem, however, that the validity of the W.C.L., although high enough for work with groups, is probably too low for it to be of more than suggestive value for individual selection and screening work.

The test would appear to have its greatest interest and use in theoretical investigations. So far it has been used chiefly in correlational studies. In the field of social attitudes, Eysenck (1951), using the W.C.L. to define neuroticism, rejected a hypothesis linking Radicalism with emotional instability. He also confirmed a correlation between neuroticism and 'toughmindedness' which had previously been reported by the writer, although in this study (Crown, 1950) the correlation just failed to be significant at the 5% level. Crown (1950) also showed, using the W.C.L., that neuroticism was significantly correlated with warmindedness, anti-Semitism and a tendency to give emphatic answers to the Social Attitudes Inventory. Loos (1951) used the W.C.L. as an aid in defining neuroticism in his investigation of sense of humour, his group consisting of seventy-six young women in a teachers' college. The W.C.L. had a correlation of -0.34 with his Social Humour rating, that is, rating of sense of humour by other people.

In addition to correlational studies, another theoretical context in which the test should be useful is in checking specific hypotheses as to the abnormality of a given group. Standen (1950), in the field of psychosomatic medicine, used the test as part of a battery to test the hypothesis that patients suffering from hyperthyroidism (thyrotoxicosis) are more neurotic than a group of control patients with no psychosomatic symptoms. Scores on the W.C.L. were in the predicted direction and, together with the evidence from other tests, served to give the hypothesis fairly good verification.

Finally, a theoretical use must be mentioned which the author has not seen discussed elsewhere. The W.C.L. may be of use where, in planning a research, it is considered necessary to match an experimental with a control group on degree of neuroticism as well as on other variables, e.g. intelligence.

IV. SUMMARY AND CONCLUSIONS

1. Normative data on over 3000 normal and abnormal persons are presented for the Word Connexion List, a controlled association test for the measurement of neuroticism. Evidence is presented to show that the test differentiates between normal and neurotic groups.

2. Psychotic (depressive and schizophrenic) groups tend to score lower on the W.C.L. than the neurotic groups. This finding supports the hypothesis that the variation from normality of the psychotic and neurotic illnesses is along different, uncorrelated, dimensions.

3. There is suggestive evidence that groups of persons suffering from certain psychosomatic disorders, namely, migraine and thyrotoxicosis, and groups of inadequate and aggressive psychopaths, score as neurotic rather than as normal or psychotic persons on the W.C.L.

4. 'Normal' samples from the general population show a considerable range in scores on the W.C.L. This finding may, in part, be due to variations in intelligence between the groups, but emphasizes the necessity for caution in drawing conclusions from the results of single investigations where the test performances of normal and abnormal groups are compared.

5. The screening efficiency of the W.C.L. is probably not good enough for it to have more than suggestive value in individual (screening and selection) work. A final decision on these practical uses of the test can, however, only be made when further studies have been done using criteria of neuroticism of a known degree of reliability.

6. For group work, the W.C.L. may have considerable theoretical value; in correlational studies, in testing specific hypotheses as to group differences in neuroticism and for matching purposes.

I am particularly grateful to Dr J. W. Lovett Doust for his advice on psychiatric questions connected with this research and for providing some of the test data; also to Dr H. J. Eysenck for some most helpful suggestions.

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APPENDIX

*Copy of Word Connexion List (abnormal responses underlined)**Word Connexion List*

Below you will find a list of familiar words printed in capital letters, each followed by two words in small letters. Look at the word SINK in the example below. Now glance at the two other words. Does the word SINK make you think more of 'wash' or of 'drown'? Draw a line under whichever word is more connected in your mind with SINK.

Example: SINK wash drown

Some people connect 'wash' with SINK and so they underline 'wash' as follows: SINK wash drown. Other people connect 'drown' with SINK and therefore they underline 'drown' as follows: SINK wash drown. Just look at the two words that follow the word in capital letters and underline the one that *you* feel is more connected in your mind with that word. WORK FAST. Don't stop to think long about any one word. Be sure not to leave any words out.

SCISSORS	nurse	cut	EATING	drinking	<u>fasting</u>
WOUND	bandages	<u>feelings</u>	SALMON	<u>dislike</u>	<u>like</u>
WEIGHT	scale	<u>losing</u>	STOMACH	<u>food</u>	<u>ache</u>
MAN	<u>work</u>	woman	LIGHT	dark	<u>sentence</u>
TROUBLE	<u>lawyer</u>	sorrow	TALKED	spoke	<u>about</u>
HEAVY	<u>weight</u>	<u>heart</u>	SLEEP	<u>nightmares</u>	<u>bed</u>
BRING	take	<u>disaster</u>	DEEP	<u>ocean</u>	<u>hurt</u>
FAIRY	<u>shameful</u>	wand	NEEDLE	<u>drug</u>	<u>sharp</u>
LOSE	<u>find</u>	<u>mind</u>	CAN'T	<u>concentrate</u>	<u>fly</u>
BABY	<u>foundling</u>	little	FOOT	<u>hand</u>	<u>tingle</u>
BITTER	<u>medicine</u>	sweet	FEEL	<u>useless</u>	<u>good</u>
THIRSTY	<u>dry</u>	drink	PARTY	<u>crowd</u>	<u>myself</u>
FOOD	stomach	<u>poisoned</u>	WOMAN	<u>excitement</u>	<u>man</u>
CONTENTED	happy	<u>discontented</u>	UNHAPPY	no	<u>yes</u>
SWIFT	<u>hurricane</u>	slow	SHORT	<u>little</u>	<u>tall</u>
BOY	girl	<u>mischief</u>	GRAVE	<u>serious</u>	<u>funeral</u>
MUTTON	eat	<u>flesh</u>	SOUR	<u>lemon</u>	<u>stomach</u>
SLOW	<u>beware</u>	fast	LEFT	<u>home</u>	<u>right</u>
THOUGHTS	<u>ideas</u>	<u>strange</u>	PINT	<u>quart</u>	<u>whiskey</u>
HUNGRY	thirsty	<u>heart</u>	FRIEND	<u>double-crossed</u>	<u>close</u>
MAN	<u>hard</u>	boy	WOMAN	girl	<u>trouble</u>
BLUE	<u>sad</u>	sky	RAW	<u>deal</u>	<u>meat</u>
LION	<u>eat</u>	tiger	SWEET	<u>affected</u>	<u>bitter</u>
RIVER	<u>lake</u>	<u>danger</u>	LOUD	<u>yell</u>	<u>soft</u>
DIGNIFIED	<u>snobbish</u>	<u>poised</u>	HANDS	<u>feet</u>	<u>moist</u>

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AN EXPERIMENTAL INVESTIGATION OF THE ABSOLUTE AND RELATIVE THEORIES OF TRANSPOSITION BEHAVIOUR IN CHILDREN

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I. INTRODUCTION

An animal trained to select the brighter of two surfaces, will select the dimmer one if it is presented together with a still dimmer surface; if trained to approach the larger of two boxes, it will approach the smaller when presented with an even smaller box. These are well-known examples of transposition behaviour. Such behaviour has been investigated in animals and humans who have learned discriminative reactions to stimulus-objects differing in size, brightness, weight, pitch and loudness: the results have been summarized by Jackson, Stonex, Lane & Dominguez⁽¹²⁾ and Warden & Rowley⁽³²⁾. The facts, however, are subject to divergent theoretical interpretations. Is the organism responding to objects in isolation (absolute theory) or to objects in relation to each other (relative theory)? The latter theory, championed by Lashley^(17, 18, 19), stresses the role of perceptual organization and asserts 'that animals often respond to ratios of excitation and that in many cases the perception of a relation is more primitive or elementary than, and prepotent over, the perception of specific magnitudes' (⁽²³⁾, p. 374). The absolute theory, most systematically formulated by Spence^(26, 27), denies both transposition and perceptual organization as basic behaviour mechanisms and seeks to demonstrate their secondary character by deducing them from other principles.¹ For clarity's sake, an account of those neo-Pavlovian principles will now be given.

The absolute theory deduces transposition behaviour from the combined principles, controversial in themselves, of conditioning and of primary stimulus generalization. The first principle treats of the association, governed by the law of effect, of stimuli with responses^(7, 26). A 'stimulus' is a physical energy capable of affecting the organism's sense organs or, at a less analytic level, a specific combination of afferent traces organized only in that the traces exist simultaneously and mutually modify each other through 'afferent neural interaction'. Discrimination learning 'is conceived as a cumulative process of building up the excitatory strength... of the positive cue as compared with the competing excitatory strength of the negative cue. This process is assumed to continue until the difference between the excitatory strengths of the two cues is sufficiently great to offset any other differences in excitatory strength that may exist between the stimulus

¹ The relative-absolute issue cuts across several distinct learning theories, among which exist other points of harmony and discord. See, for example, Kohler⁽¹⁴⁾, Gulliksen & Wolfe⁽⁴⁾, Lashley^(18, 19), McCulloch⁽²¹⁾, Spence^(26, 27) and Hull⁽⁷⁾.

complexes of which the cues are members' (29), p. 719). It is not the relation which is important but the 'stimuli' themselves. The second principle is postulated and elaborated by Hull (6, 7, 8) to account for cases of stimulus equivalence which cannot be explained either by partial physical identity of the stimulus complexes involved or by mediation between those complexes by a response conditioned to both. It is a sophisticated formulation of Pavlov's 'irradiation' (24). It states that when a reaction is conditioned to a 'stimulus' at one point on a continuum,¹ there is an automatic spread of training effect to adjacent points, the effect varying inversely with distance from the conditioned point. After discrimination, both the 'excitatory strength' of the active stimulus and the 'inhibitory strength' of the inactive stimulus have generalized along the continuum; the excitatory strength at any point is the algebraic sum of the generalized excitatory and inhibitory strengths at that point. Fig. 1 illustrates the way in which the two principles

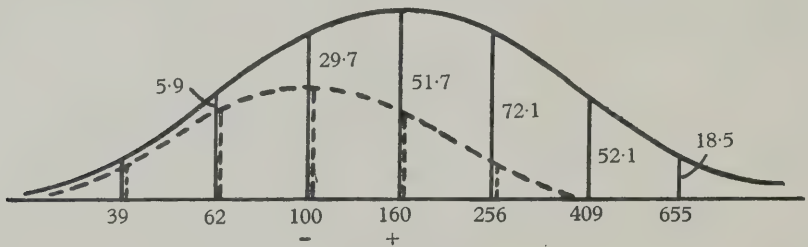


Fig. 1. Diagrammatic representation of the absolute theory. The stimuli are squares of different areas, measured in sq.cm. and plotted at equal distances along a logarithmic scale. The subject has been trained to select square 160 in preference to square 100; the solid and dotted vertical lines represent, respectively and proportionately, the excitatory and inhibitory strengths at those points. The excitatory and inhibitory generalization gradients are indicated by the solid and dotted curves and their algebraic sum at each point is shown (after Spence (28), p. 258).

combine to explain transposition behaviour. Since the stimulus preferred at any moment is that with the greatest excitatory strength, it is apparent that 62 is preferred to 39, 100 to 62, 256 to 160 and 409 to 256. The predicted breakdown of transposition with the pair farthest removed from the training pair is in accordance with experimental findings. In this way is deduced not only transposition's occurrence but its failure.

The absolute theory has two versions which, while making like assumptions about stimuli and transposition, differ over the responses involved in discrimination. That presented above assumes the operation of excitatory and inhibitory strengths of the habits linking the *same* response to each stimulus and may be called the one-response version. The two-response version differs in that 'the negative stimulus becomes conditioned to a new response, incompatible with approach, as contrasted with the notion that Pavlovian inhibition is built up in regard to the negative stimulus' (21), p. 80). It therefore substitutes for competing excitatory and inhibitory strengths of the *same* response, competing excitatory strengths of *different* responses.

The present study, using young children as subjects, aims at an experimental assessment of both the relative-absolute issue and of the one- and two-response version of the absolute theory.

¹ The concept of stimulus continuum or dimension is used by all theorists discussing transposition. Boring (2) has given the concept its most detailed exposition.

II. EXPERIMENT I

(i) *Introduction*

Let A, B, C, D, E, F, G and H represent eight stimulus-objects identical in all respects except one, regarding which they vary in an orderly way such that the psychological difference between A and B equals that between B and C, C and D , and so on. Let X represent a stimulus-object which falls nowhere on the above continuum. In the experiment, the subject learns three discrimination habits, each involving a differential response to one of two simultaneously presented objects, and is then given 'critical trials', i.e. tested for transfer to a new pair of objects. Indicating the objects to which the differential response is made by plus signs and the others by minus signs, the plan of the experiment is:

Habit I	$D(+)$ v. $A(-)$	Habit III	$C(+)$ v. $X(-)$
Habit II	$F(+)$ v. $C(-)$	Critical pair	E v. H

The relative and absolute theories differ over the member of the critical pair which will be chosen.

For the absolute theory, Habits I and II set up excitatory strengths at points D and F and inhibitory strengths at points A and C , while Habit III establishes an excitatory strength at C sufficient to overcome the inhibition already there as a result of direct conditioning and of inhibitory generalization from A . The net effect of training, all gradients considered, is to establish excitatory strengths at C, D and F (F 's strength exceeds C 's since F is preferred to C in Habit II; if transposition is to occur, D 's strength is intermediate between those of F and C). Which has the greater excitatory strength, E or H ? Since neither entered directly into the training situation, their excitatory strengths derive entirely from generalization effects. H is affected less than E by generalization from F because the distance between F and H is twice that between F and E . Likewise, generalizations from C and D affect the farther point, H , less than the nearer point, E . In consequence, E is preferred to H . This prediction is independent of finical assumptions about the shapes of the generalization gradients involved. Excitatory strength must either increase, remain steady or decrease with greater distance from the point conditioned and, the first two possibilities being incompatible with discrimination, the last must be presumed to operate. All varieties of such a diminishing-type gradient give the same prediction—except one. That exception arises when generalization is restricted within a single step, i.e. when F has no generalized effects on either E or G , when D has no effects on either C or E , etc. Under such conditions, H is chosen as often as E in the critical trial—but the theory has become incapable of explaining transposition. The choice of E is the only one possible on absolute principles.¹ In terms of the relative theory the presentation of the critical pair restores the relational (and ratio) situation present during the acquisition of Habits I and II and the subject, reacting to this relation as in training, prefers H to E .

The absolute theory's prediction could have been deduced from either the one- or

¹ The critical trials could follow directly on Habits I and II were it not possible for inhibitory strengths at A and C to generalize to E to make its excitatory strength less than H 's. This reversal of prediction results from (i) experimental extinction being carried beyond zero point, and (ii) generalization gradients being of arbitrary shape. The function of Habit III is to ensure an excitatory strength at C and enable us to disregard inhibitory generalizations from either A or C . This could be accomplished by making Habit III, say, $C(+)$ v. $A(-)$, but, again, elusive generalizations from A might operate. Using a stimulus-object outside the continuum as the negative member of Habit III is the most efficient way of fulfilling that habit's purpose.

two-response versions. Should that prediction be verified, could the merits of the two versions be assessed? The expectations of the one-response version regarding the course of training are not clear, but those of the two-response version oppose those of the relative theory. The two-response version predicts, since *C* is to be approached in Habit III but avoided in Habit II, negative transfer from Habit II to Habit III with a reciprocal negative transfer from the latter, disrupting the two Habits already acquired. Simultaneous existence of all three habits requires extensive training. For the relative theory, Habit III, lacking the relational properties of the first two habits, presents a new learning situation and gives rise to little if any transfer, either negative or positive, between the first two habits and the third.

(ii) *Subjects and apparatus*

The subjects were thirty-two infant-school children (eighteen boys and fourteen girls, aged 59–63 months).

The stimulus dimension was that of size.¹ The stimulus-objects were 6 × 6 in. matt black squares, cut out of stiff cardboard. In the centre of each was a matt white circle which varied in size from card to card and representing the circles by the letters *A*, *C*, *D*, *E*, *F* and *H*—had radii of 1.0, 1.4, 1.6, 2.0, 2.2 and 3.2 cm. respectively. Those measurements fall at equal intervals along a logarithmic physical scale. The ratio of areas *D* : *A*, *F* : *C* and *H* : *E* was 2.56 : 1.00. A seventh card, *X*, the same size as the others, was painted uniform matt black.

The presentation-box, constructed of $\frac{1}{4}$ in. wood, measured 15 in. in length, 7.5 in. in height and 6.5 in. in the breadth of its base. On it, two cards could be presented securely always in the same position (1 in. apart) and at an angle of 60° from the horizontal. Beneath each card was a small drawer which could be opened but not removed from the box. The back of the box was open, so that rewards could be placed in the drawers from behind. Two apertures, cut in the box, enabled the experimenter to see, during presentation, the identifying letters printed on the backs of the cards. The front and top of the box were painted matt black. The rewards were raisins.

(iii) *Procedure*

The experiment was conducted, during school hours, in the building of the school attended by the children. The room used was quiet, free from interruptions and had good daylight illumination. Experimenter and subject sat on opposite sides of a table on which the presentation-box was placed so that the stimulus-cards were at, or slightly below, the level of the subject's eyes and about 1 ft. distant from them. When the subject was brought into the room, the cards for Habit I were in the presentation position, and all other cards were lying face down on the table. The subject was seated in front of the box and instructed: 'Now, we are going to play a game. You like playing games? Well, here are two cards (point to each in turn) and one of them is right and one of them is wrong. Each time, you have to look at both cards and see which is right for, if you pull out the drawer—look, there's a drawer here and another here—beneath the right card, you get a raisin

¹ If size is regarded as a 'relative' property (2), the absolute theory of transposition fails from the outset. However, size is 'relative' only in that it requires, in addition to sensory stimulation from the object itself, the simultaneous presence of a certain combination of diverse other stimulations to make up a specific pattern. This pattern may be thought of as 'absolute'. Conversely, 'absolute' may often be treated as 'relative' within a wider frame of reference (see Van Beusekom's study (31) of spatial orientation in *Sphegid* wasps).

You like raisins? If you get the raisin, you may keep it or eat it or do what you like with it. If you pull out the drawer under the wrong card, there will be no raisin there and you won't get any. Now, let's pull out the drawer under this card. (Pull out drawer beneath wrong card.) There's nothing there so that must be the wrong card. Now, let's pull out the drawer under this card. (Pull out drawer beneath right card.) Oh, look, there's the raisin. Take it. (Wait till the subject takes it.) Sometimes the raisin's in this drawer and sometimes it's in the other drawer, and the only way you can tell which one it's in is to look at the cards and see which is the right one.'

The two drawers were closed and the experimenter took his seat behind the presentation-box at the other side of the table. After noting down the time and the subject's name on the prepared blanks, the experimenter placed a raisin in the drawer beneath the positive card and told the subject to 'look at both of the cards and see which one you think is right, then open the drawer beneath it'. If the 'right' drawer was opened, the subject took the raisin. If the 'wrong' drawer was opened, the experimenter held the other from behind so that the subject could not open it, and informed the subject that he must open the 'right' drawer first time otherwise he could not get the raisin. This was the first trial. For the second trial, the cards' positions were changed, a raisin placed in the 'right' drawer and the subject told 'Choose'. The same two cards were repeatedly presented until the positive one was chosen in nine out of ten consecutive trials. Habit I was then regarded as learned. Without indication of change in procedure, training was given on Habit II until the criterion of nine correct choices out of ten was reached. Training was then given on Habit III until nineteen out of twenty trials were correct. This ended the training procedure.

The five following precautions were taken against vitiation of the results by possible extraneous factors: (1) Size-preferences. Half of the subjects were trained to choose the larger and half the smaller circles. (2) Differences in learning when the training pair is at the large or at the small end of the size continuum. Half of the subjects were trained on the large pair first and half on the small pair first. These first two precautions gave rise to four groups of subjects with different training-testing schedules as shown in Table 1. The

Table 1. *Showing the stimulus pairs presented to the four training groups*

	Group I	Group II	Group III	Group IV
Habit I	$E(+)$ v. $H(-)$	$C(+)$ v. $F(-)$	$D(+)$ v. $A(-)$	$F(+)$ v. $C(-)$
Habit II	$C(+)$ v. $F(-)$	$E(+)$ v. $H(-)$	$F(+)$ v. $C(-)$	$D(+)$ v. $A(-)$
Habit III	$F(+)$ v. $X(-)$	$F(+)$ v. $X(-)$	$C(+)$ v. $X(-)$	$C(+)$ v. $X(-)$
Crit. Pair	A v. D	A v. D	E v. H	E v. H

allocation of eight subjects to each group was not based on either chronological or mental age, but an uneven distribution of the most and least bright subjects (teacher's estimate) was avoided. (3) Various position habits. The positions of the cards were altered randomly from trial to trial. The side (right or left) on which any one member of a pair appeared on successive presentations was R L R R L L R L L R L L R R L R R L. The alteration was performed by removing the cards, in full view of the subject, lifting them down behind the experimenter's side of the table where changes of position or of cards themselves could be effected out of the subject's sight, and returning them to the presentation-box. This removal and replacement of cards was performed between all trials even when no changing was required. (4) Differential visual or auditory cues given by the experimenter while placing the reward in one of the drawers. The experimenter put forward both hands to the raisins, kept in a heap behind the box, and then to the drawers, one hand to each, and left,

on withdrawing his hands, a raisin in the appropriate drawer. (5) Differential cues from incidental features of the cards not apparent to the experimenter. In addition to careful preparation of the cards, two precautions were taken. First, the critical cards did not appear during training, so that dependence on accidental cues would reveal itself in chance selection during testing. Secondly, the cards used in training for each separate habit were replaced, two trials before the criteria were met, by corresponding cards from a duplicate set with a slightly different (egg-shell) finish. None of the first twelve subjects showed a habit-breakdown or even a recognition of change when these substitutions were made. Thereafter, substitution from the duplicate set was discontinued.

Whether or not the subject understood the verbal instructions, he usually came, within one or two trials, to make his choice without guidance from the experimenter other than the signal—the word ‘Choose’—when all was ready. In the case of a few subjects who persisted in opening drawers without reference to the cards above them, the response was broken up into, first, looking at the cards, secondly, choosing one of them, and thirdly, opening the drawer beneath the chosen card. A few such fragmented responses sufficed to base the subject’s choice on the cards without the experimenter’s further assistance. The subjects were friendly, co-operative and interested in the ‘game’ throughout.

Testing was carried out immediately as though it was just another training trial. The critical pair was presented, in different positions, for five trials, during which a raisin was present in each drawer so that choice of either was rewarded. To determine whether the later habits affected the retention of the earlier ones, half of the subjects in each group were given six testing trials with the training pairs before being given the critical presentations. The six trials comprised two presentations of each training pair with each card once on the right and once on the left and choice of either rewarded.

The time between trials was no longer than was required by the experimenter to note the subject’s choice and prepare the next presentation. There were, on the average, about four presentations per minute.

There was no talking during the experiment. Spontaneous comments by the subject, were, if relevant, recorded. Interrogation was confined to a single question which was put on four occasions. The question was: ‘You always know which one to choose. Which one do you choose?’ It was asked two trials before the criteria for Habits I, II and III were met and after the five critical trials. Verbal reply was encouraged by having all cards out of sight during interrogation.

(iv) *Results*

The choices made on the first critical trial are summarized in Table 2. Of the five who chose ‘absolutely’ on the first trial, four chose inconsistently (i.e. did not always choose the same card) on the remaining trials, two adopting position habits and two seeming to

Table 2. *Showing choices made on the first critical trial*

	Group I	Group II	Group III	Group IV
No. of subjects choosing ‘absolutely’	1	1	2	1
No. of subjects choosing ‘relatively’	7	7	6	7

choose at random. The subject who responded both ‘absolutely’ and consistently belonged to group I. He reported choosing, in Habits I and II, the ‘wee one’.¹ In Habit III, he

¹ The word ‘wee’ means ‘small’, and is not only used in the Scottish child’s everyday vocabulary but encountered by him in his school reading books.

described his choice as the 'big one' and, when shown the critical pair, chose the 'big one' again. After the critical trials, the original training pairs were re-presented and he was found, contrary to training, to choose the larger circles. This, coupled with 'absolute' responding in the critical trials, is discordant with the absolute theory. Of the 27 subjects choosing 'relatively', only two gave inconsistent choices as testing proceeded. Therefore of the thirty-two subjects, twenty-five clearly behaved, in the critical trials, as the relative theory would predict, while the remaining seven gave inconsistent responses. Each of those seven was found, when re-presented with the training pairs either before or after the critical trials, to have lost his learned discrimination habits. The confusion of choice may thus be attributed to breakdowns of the originally learned habits.

As the absolute theory has been found inadequate, it is unnecessary to inquire which of its versions is the better. Nevertheless, consideration of the validity of the opposing predictions about training provides an interesting cross-check on the relative-absolute issue. The total of all errors made by all subjects while learning Habit I was 124, the total for Habit II was 3 and, for Habit III, 15. Both the relative theory and the two-response version of the absolute theory expect the almost perfect transfer from Habit I to Habit II (thirty-one of the thirty-two subjects transposed on the first trial of Habit II). However, the two-response version predicts a greater error score on Habit III than actually occurred. It also predicts choice of the black-card on the first presentation of the pair for Habit III since the circle-card has acquired an avoidance tendency. In fact, twenty-seven of the thirty-two subjects chose the circle-card on the first presentation of Habit III, demonstrating, thereby, a lack of negative transfer from the first two habits to the third. Nor does Habit III unduly disturb Habits I and II. Of the sixteen subjects (four in each group) given testing trials with the three training pairs immediately before the critical trials, only two, both of whom responded inconsistently to the critical pair, showed a breakdown of the previously learned habits. Thus, the relative theory's predictions hold over those of the two-response version of the absolute theory.

Only twenty-four subjects were interrogated, since questioning was inadvertently omitted with the last eight subjects. Of the twenty-four, twenty chose consistently and 'relatively' in the critical trials. Only eight did not express their choices verbally—six of those chose 'relatively' and two 'absolutely'. Of the twelve subjects trained to choose the smaller circles in Habits I and II, eleven verbalized the relation, while of the twelve trained to the larger circles, only five verbalized the size-relation.

(v) *Summary*

Under the incentive of obtaining a raisin and otherwise playing a game, thirty-two children, aged 59–63 months, each learned three successive discrimination habits involving circles of different sizes. These circles are referred to by their areas, given in sq. cm. Half of the subjects were trained as follows: Habit I, 8.1(+) v. 3.1(–); Habit II, 5.1(+) v. 6.2(–); Habit III, the negative member of the last pair is positive and a card with no circle on it is negative. These subjects were given critical pair, 12.4 v. 32.2. The other half of the subjects were trained: Habit I, 12.4(+) v. 32.2(–); Habit II, 6.2(+) v. 16.1(–); Habit III, the negative member of the last pair is positive and a card with no circle on it is negative. These subjects were given critical pair, 8.1 v. 3.1. The relative theory predicts that, in the critical trials, members of the first half will choose the larger circle and members of the second half will choose the smaller circle. The absolute theory

predicts choice of the other circle in each half. Choices, clearly and consistently in accordance with the relative theory, were made by twenty-five of the thirty-two subjects, while those of the other seven were equivocal because of breakdowns of the learned habits. The two-response version of the absolute theory expects, during training, strong negative transfer between Habits II and III in both groups of subjects. The performances, during training, revealed no such negative transfer. These results support the relative theory of transposition.

III. EXPERIMENT II

(i) *Introduction*

The previous experiment showed that transposition depends on a stimulus relation of some sort. The present experiment investigates the nature of this relation and inquires into the roles of qualitative relational and absolute properties of the stimulus-objects, of the quantitative ratios between the objects, of the amount of training and of pre-existent size-preferences.

(ii) *Subjects and apparatus*

The subjects were fifty-five infant-school children (thirty boys and twenty-five girls) aged 60-71 months, none of whom had acted in the previous experiment:

The apparatus differed from that described in Exp. I only in the sizes of the circles used. Representing the circles by the letters *T*, *U*, *V*, *W*, *X*, *Y* and *Z*, their radii were 0.7, 1.0, 1.4, 2.0, 2.8, 3.9 and 5.45 cm. respectively. The ratio of the areas *T* : *U*, *U* : *V*, etc., was 1 : 2.

(iii) *Procedure and results*

Verbal instructions, method and order of presentation and system of rewards were as in Exp. I. The detailed procedure is summarized, along with the main results of the experiment, in Table 3. Each subject was allocated randomly to one of six groups. The

Table 3. *The training procedures and results of Exp. II*

Group no.	No. in group	Training pair	Testing pairs	
			After 10 post-fixation trials	After 30 post-fixation trials
1	10	<i>Y</i> (+) <i>v.</i> <i>Z</i> (-)	<i>X</i> (7) <i>v.</i> <i>Y</i> (3)	<i>T</i> (10) <i>v.</i> <i>Y</i> (0)
2	10	<i>Y</i> (+) <i>v.</i> <i>Z</i> (-)	<i>T</i> (7) <i>v.</i> <i>Y</i> (3)	<i>X</i> (10) <i>v.</i> <i>Y</i> (0)
3	9	<i>Z</i> (+) <i>v.</i> <i>Y</i> (-)	<i>X</i> (0) <i>v.</i> <i>Y</i> (9)	<i>T</i> (0) <i>v.</i> <i>Y</i> (9)
4	8	<i>U</i> (+) <i>v.</i> <i>T</i> (-)	<i>V</i> (8) <i>v.</i> <i>U</i> (0)	<i>Z</i> (8) <i>v.</i> <i>U</i> (0)
5	8	<i>U</i> (+) <i>v.</i> <i>T</i> (-)	<i>Z</i> (8) <i>v.</i> <i>U</i> (0)	<i>V</i> (8) <i>v.</i> <i>U</i> (0)
6	10	<i>T</i> (+) <i>v.</i> <i>U</i> (-)	<i>Z</i> (2) <i>v.</i> <i>U</i> (8)	—

table's first column shows the number of the group and its second the number of subjects in each group. In the third column is the pair of cards on which training was given, which member of the pair was positive and which negative. When the subject had chosen correctly in nine out of ten consecutive training trials, he was presented twice with the critical pair shown in column four. After twenty more training trials, making a total of thirty post-fixation training trials, the subject was given two presentations of the critical pair shown in the last column. Only one critical pair was used with group 6. The number in brackets after each member of the critical pairs represents the number of the subject choosing the card on the first critical trial. In only five cases was the card chosen on the second critical trial different from that chosen on the first. One subject, in group

presented with X and Y , chose X on the first trial and Y on the second, while another subject, in the same group, chose Y first and then X . Two subjects, in group 2, presented with T and Y , chose first T and then Y . One subject, in group 6, chose Z on the first critical trial and T on the second.

Table 3 shows that transposition is independent of strict maintenance of the ratio between the areas of the two circles constituting the training pair. This ratio might be increased sixteen-fold without destroying transposition, for, although this ratio in the test trial circles of groups 1 and 4 was 1 : 2 and of groups 2 and 5 was 1 : 32, there was no difference in the number of transposition responses. However, just as transposition breaks down if the critical pair is too far removed from the training pair (1, 16) so, too, the ratio presumably reaches a limiting size beyond which transposition does not occur. Indeed, the two subjects of group 2 who chose inconsistently pointed toward less transposition with the novel ratio $T : Y$ than with the preserved ratio $X : Y$ (in group 1).

In groups 1 and 2, the total number of non-relational choices was six after ten post-fixation trials, and zero after thirty trials. Practice, it seems, gave stability to the relational choice. Support for such a suggestion comes from an exploratory study (unpublished) by the author in which fourteen children, aged 27–40 months, were trained to choose the smaller of two square wooden blocks and, after ten post-fixation trials, given six critical trials with a smaller pair five steps removed from the training pair. Six subjects transposed on those critical trials and, of the remainder, another six transposed after an additional twenty training trials. Further support comes from an investigation by Jackson *et al.* (12) of transposition in young children. Speaking of practice-effects, he concludes: 'Following a small amount of post-fixation practice of a discrimination habit, choice between transposed stimuli will be confused and unstable with frequent reverting to position habits. Following a greater amount of practice, choice upon transposition is consistently made on a relational basis. After a still greater amount of practice some subjects, at least, will make choice on an absolute basis' (12, p. 598). This third stage was, obviously, not reached in the present experiment.

Since, after thirty post-fixation trials, the critical choices were all relational, evidence or other influences must be sought in the earlier critical trials. In groups 1 and 3, subjects were trained on Y and Z with the former card positive in group 1 and negative in group 3. Both groups were tested on X versus Y but, while group 3 gave 100 % relational responding, group 1 gave only 70 %. Granted that the groups are equated, this difference has two possible explanations. First, training may not have established a transposable relational habit in every case and the subjects concerned made their critical choices on the basis of a pre-established size-preference; this would give an 'absolute' choice in group 1 and a 'relative' choice in group 3. Secondly, training may have established a habit which, while primarily relational, possessed an absolute component in the form of a tendency to choose the positive member *per se*. In the test trials of group 1, an absolute choice was available but, in those of group 3, an absolute choice (i.e. card Z) was impossible, and the relational component would be left to exert its unopposed influence. Which of these explanations is it to be? All three following observations support the first possibility. (1) In groups 4 and 5, an absolute choice is available in each critical pair, yet every subject made a relational choice. In each group, the operation of a size-preference would lead to a 'relative' choice. (2) In the critical trials of group 6, *both* the relational and

absolute choice would be *T*, and choice of *Z* would indicate an offsetting preference for the larger circle. Of the ten subjects, two chose *Z* in the critical trials. (3) If a size-preference is operative, it would be expected that, taking the subjects as a whole, there would be fewer errors made in learning to choose the larger circle than in learning to choose the smaller. The training performances of subjects from this and the previous experiment were pooled (despite slight differences in the ratios between the areas of the circles in the two). The forty-six subjects trained to choose the smaller circle had a total of 271 errors with individual scores ranging from 0 to 25 and averaging 5.89. The forty-one subjects trained to the larger circle had a total of 145 errors with individual scores ranging from 0 to 23 and averaging 3.54. Not only did subjects learning to choose the larger circle make but 60% of the errors of subjects learning to choose the smaller, but the percentage of the former incurring less than any particular error-score was consistently greater than the percentage of the latter. Reports have already been made by other investigators (5, 9, 33) of preferences for the larger stimulus-object in children's discrimination learning.

Combining the verbalization results of Exp. I and II, seventy-nine subjects had opportunity to describe their choices in words. Of the thirty-seven subjects trained to choose the larger circle, sixteen (43%) gave their choice verbal expression. Of the forty-two trained to choose the smaller circle, twenty-nine (69%) verbalized their choice. Applying χ^2 , the difference between the groups was found to be significant beyond the 0.005 level of probability. Thus, while the subjects preferred the larger circle to the smaller, they gave more ready verbal expression to the latter. In view of the usual assumption of a single large-small relation subsumed beneath the heading of 'logical relation of opposition' (15), it might have been expected that there would be no difference between the proportions of verbalizers in the 'larger group' and the 'smaller group' and that the results were due to the groups being unmatched as regards the verbal ability of their members. However, it would also be expected that the better verbalizers would be the better learners (25). But this was not borne out, for the 'larger group' contained the better learners and the 'smaller group' the better verbalizers. The results seem genuine and not artifacts of unmatched grouping. Interpretation will not be attempted, but the findings left as an interesting pointer to a problem in need of investigation.

(iv) *Summary*

Fifty-five children, aged 60-71 months, were split into six groups, trained on different size-discrimination habits and tested for transposition to various other size-pairs. The apparatus and method was essentially as in the previous experiment. Two critical trials were given after ten post-fixation training trials and another two after thirty post-fixation training trials. The results were as follows. (1) Transposition depends on the qualitative relationship (e.g. larger, smaller) between the members of the training pair more than on the quantitative ratio (e.g. twice as large, a third as small) between them. (2) Absolute properties of individual circles were not found to influence the type of transfer. (3) Practice gives stability to relational responding. (4) The subjects have pre-established preferences for larger circles. In training, this makes learning to choose the smaller more difficult than learning to choose the larger circle. In testing, it manifests itself through choice of the larger circle in cases where transposition fails to occur. (5) The subjects give more ready verbal expression of the choice of the smaller circle than of that of the larger.

IV. EXPERIMENT III

(i) *Introduction*

The inadequacy of the absolute theory's explanation of transposition behaviour in five-year-old children was demonstrated in the first experiment. But, of adherents to the absolute theory, Spence has been notable in assuming non-articulate organisms, i.e. animals and pre-linguistic children. The discrimination behaviour of organisms capable of verbal processes may become cued to such words as 'smaller', 'brighter' and 'heavier'. Kuenne, working in Spence's laboratory, gives succinct expression to this view: 'that the mechanisms assumed by Spence to underly this behaviour in animals are also operative in the young child, and that with the development in older children of the capacity to employ verbal responses in such behaviour situations, a shift occurs to the verbal type of control' (16, p. 474). The subjects studied were already of an age when language might make a considerable difference in the transposition tests. The present experiment repeats Exp. I, using 'pre-verbal' children as subjects.

(ii) *Subjects and apparatus*

The subjects were fifteen day-nursery children. Of the twelve who completed the experiment (eleven boys and one girl) the ages ranged from 14 to 32 months with a mean of 23.1 months.

The stimulus-objects comprised six wooden blocks, each 1.5 in. deep, hollowed out in the centre of the under-side, painted neutral grey and square in shape. Denoting the blocks by the letters *A*, *C*, *D*, *E*, *F* and *H* (blocks *B* and *G* of the series are not required), the areas of their square surfaces were 4.0, 10.2, 16.4, 26.2, 41.9 and 107.4 sq.in. respectively. Thus, the area ratios of *A* : *D*, of *C* : *F* and of *E* : *H* were each 1 : 4.1.

(iii) *Procedure*

The three discrimination habits were given simultaneous rather than, as before, successive training. The subjects were divided into two groups—SM and LA—the training-testing schedules for which are shown in Table 4. The presentations were made

Table 4. *Training-testing schedule for Experiment III*

	Habit I	Habit II	Habit III	Critical Pair
Group SM	<i>E</i> (+) v. <i>H</i> (-)	<i>C</i> (+) v. <i>F</i> (-)	<i>F</i> (alone)	<i>A</i> v. <i>D</i>
Group LA	<i>D</i> (+) v. <i>A</i> (-)	<i>F</i> (+) v. <i>C</i> (-)	<i>C</i> (alone)	<i>E</i> v. <i>H</i>

so that (1) trials on Habits I, II and III occurred in the ratio of 2 : 2 : 3 and in a randomized order, and (2) any single member of any one pair appeared equally often on right and left. Although the one- and two-response versions of the absolute theory are not being examined here, the greater amount of practice on Habit III was retained, since, while superfluous, it makes the absolute theory's prediction all the more emphatic. As in the first two experiments, the rate of presentation averaged about four per minute.

The experiment was conducted in a screened-off area at one end of a long playroom. Noises from the other children, although present, did not seem to disturb the subject. Unless resistance was offered, the subject was led by the hand to the experimental 'room' where he was seated in a baby chair in front of a small square table (top measured 12×20 in.). The experimenter sat on a low stool at the opposite side of the table with the

blocks on the floor by his side and out of the subject's sight. A dried currant, taken from a bag beside the blocks, was placed in the subject's mouth. When this was eaten, a second currant was placed on the table in front of the subject who was encouraged to pick it up and eat it. Next, a currant was placed before the subject and covered by a block (that to be used in Habit III) which all subjects played with but none attempted to lift. The experimenter from behind, tilted back the block so that it stood on one of its edges and left the currant exposed for the subject. When the block was again presented, the subject usually attempted to lift the block, and if, as frequently happened, this attempt was made with only one hand, the experimenter placed the subject's unemployed hand on the block's other edge and assisted him to lift it. In two to six trials, the subject learned to lift the block and get the reward. Since the subject was physically unable to lift any but the smallest and lightest blocks, the experimenter always assisted unobtrusively by tipping up the block from behind. This aid did not provide differential cues, for it was withheld till the subject was attempting to lift the block of his choice. None of the subjects seemed to notice the experimenter's assistance. When the subject had learned to lift the presented block and take the currant, the preliminary training was complete.

At the beginning of each trial, a currant was placed on the table and covered by the appropriate block or blocks, care being exercised in the two-block-presentations against the subject's seeing which block hid the currant. After a pause, the blocks (or block) were pushed within the subject's reach. In Exp. I, correction—choosing the positive member after selecting the negative—was not permitted, but here it was allowed or not at the experimenter's discretion, since a rigid scheme was not found optimal for learning. Thus the subject might persistently and indiscriminately lift either block at first and, if unrewarded, lift the other. A few non-correction trials made such a subject more cautiously attentive and hastened learning. On the other hand, withdrawing the blocks after a wrong choice made some subjects shrink back timidly on the next trial and refuse to choose unless coaxed. But this study aims only to establish the discrimination habit by the shortest means, so it matters little whether correction was permitted or not. Training continued until nine out of ten discriminations (single-block-presentations excluded) were correct. The critical pair was presented immediately, twice in succession, each block appearing once to the left and once to the right and each covering a currant so that choice of either was rewarded. With one exception, both training and testing were carried out in a single session. The subjects were not interrogated but were asked several times, at the end of the experiment, to hand the 'big one' and the 'wee one' to the experimenter.

(iv) *Results*

Of the fifteen subjects, three did not learn the discrimination habits. Two were interested only in playing with the blocks, and one could not be induced to lift merely one block instead of both. The remaining twelve subjects reached the fixation-point in an average of thirty-five trials (twenty two-block and fifteen single-block presentations). Of the twelve ten responded, in both critical trials, in accordance with the relative theory. The exceptions chose 'absolutely' on one of the critical presentations, but both chose 'relatively' when given two further critical trials. Neither subject, therefore, reversed the trend to choose 'relatively'.

But, were those subjects truly 'pre-linguistic'? Could words such as 'large' or 'small' not be used as cues in the critical choices? Although they obeyed a few simple often

repeated commands such as 'Eat it up' or 'Give me', not one complied with the experimenter's requests, at the end of the experiment, of 'Give me the big one' and 'Give me the wee one'. The earliest age at which compliance with those requests was found was—although the writer did not examine a large sample of children—36 months. Dixon⁽³⁾ gives results which tend to support this finding that words denoting size are not understood before 3 years, and Thompson⁽³⁰⁾ reports that, when children are asked to point to the 'big one' of two lines, evidence of discrimination does not appear before the third year. It seems that, for present purposes, the subjects were 'pre-linguistic'.

(v) *Summary*

The part of Exp. I dealing with the relative-absolute issue was repeated with younger children. The stimulus-objects were square wooden blocks of different sizes beneath which a currant (the reward) could be hidden. Twelve children, of average age 23 months, completed training; three did not. The critical choices of the twelve subjects confirmed the predictions of the relative theory and demonstrated the absolute theory's inadequacy in explaining transposition behaviour in 'pre-linguistic' children.

V. DISCUSSION

We have investigated, above, the types of transfer which arise when children are trained in discrimination habits involving stimulus-objects differing only in size and are then presented with other objects which, likewise, differ only in size. Can those types of transfer tell us to what features of the environment—to what 'critical pattern' of stimulation—the children are responding?

The experiments' results may be explained by assuming that the 'critical pattern' possesses both absolute and qualitative relational properties of the training pair. The relative nature of the second component is suggested by Exps. I and III and its non-quantitative nature by Exp. II. Exp. II also implies the first component, for since the subject can respond with consistent success to the training pair before being able to transpose to the critical pair, the habit must, at first, be specific to the training situation, i.e. depend on absolute properties of the training pair. However, these properties were not found to be of the individual members, so they must be of the pair, as a pair. A 'critical pattern' with the above relative and absolute features could explain the decrease in transposition with greater difference of the test pair, in any of its aspects, from the training pair.

What of changes brought about in the 'critical pattern' by practice? It cannot be said precisely what those changes involve, but their effect is to enable the relative component, in the partial absence of the absolute component, to lead to behaviour which, formerly, required the presence of both components. Kuenne⁽¹⁶⁾, training children of mental ages 2-76 months on a size-discrimination problem to the same post-fixation criterion for each child, found a direct relation between mental age and transposition behaviour in the far test situation reminiscent of the increase in ability to solve problems by 'learning principle' (see Munn's review of such abilities in⁽²²⁾, pp. 431-5). Kuenne's finding has been verified by other investigators⁽¹⁾, and suggests increasing importance of the relative component not only with greater intra-problem practice but with more advanced developmental level. Consistent with this is the positive correlation existing between the tendency to make consistently relative choices and the speed of the child's original

learning⁽¹³⁾. Had Kuenne's younger subjects had more post-fixation practice than the older (they required some 5 times longer than the latter to reach the same number of correct post-fixation trials), her findings might have been less striking, but they would probably still have been present, for the relative component may well achieve an importance in older children unattainable, with any degree of practice, at a younger level. This trend may be thought of as reaching its climax at the complex levels of 'abstract thought' where the absolute component may be dispensed with entirely. It would, however, be wrong to assume that practice merely emphasizes the relative component, for Jackson *et al.*⁽¹²⁾ demonstrated that increasing familiarity with the training pair may lead to a choice dependent on the absolute size of its positive member.

The old question asks: which are more important—relative or isolated features? The following observations tend to answer that question in favour of the relative features. (1) An exploratory study by the writer of the solution by children, aged 3–4 years, of the middle-size problem revealed that, where a solution was reached, it was based on the absolute properties of the positive stimulus-object; the problem's difficulty seemed to lie in the utilization of isolated size-properties. (2) Jackson & Jerome⁽¹¹⁾ taught bright and dull children to discriminate between two squares of different size by presenting each square by itself, the one about 5 sec. before the other, and requiring the subject to make a response (right or wrong) after each. As expected, tests for transposition in which the squares were presented simultaneously gave negative results. But, whereas the bright subjects chose the square which had been positive during training, the dull subjects chose inconsistently, i.e. the relative features of the test situation disrupted the absolute choice at the lower but not at the higher developmental level. (3) Line⁽²⁰⁾ reports, of children's brightness discriminations, that the 'proportion of absolute to relative choices was diminished in all cases of delayed memory; i.e. where the subject had made the absolute choice in the test reaction immediately following the training period, he frequently responded to the relative stimulus if the test were repeated after a day's interval' (⁽²¹⁾, p. 81). These three studies point to comparative difficulty in utilizing absolute as compared with relative size. On the other hand, where the sizes of the members of the training pair are very different (writer's observation; similar phenomenon reported by Line⁽²⁰⁾ for brightness) or their presentations are separated in time^(10, 11), the isolated features of the objects are more important. Whether, then, objects are more readily reacted to in relation or in isolation depends on the nature of the objects and the situation in which they appear.

VI. SUMMARY

On the transposition issue, contemporary theorists subscribe either to (1) the relative theory which assumes the operation of a perceptual-cognitive organization in producing relational stimulus patterns upon which performance may be based, or to (2) the absolute theory, with its one- and two-response versions, according to which transposition and perceptual organization are secondary phenomena which may be deduced from neo-Pavlovian principles. In Exp. I, thirty-two children, aged 59–63 months, were trained in several size-discrimination habits and given critical trials which brought the predictions of the relative and absolute theories into conflict. The experiment was repeated with twelve children, aged 14–32 months, in Exp. III. The results of both experiments supported the relative theory and demonstrated the inadequacy of the absolute theory at even the 'pre-linguistic' level of development. Exp. I was also designed to test the opposing

redictions of the two-response version of the absolute theory and of the relative theory regarding the types of transfer between the different habits. The results, again, supported the relative theory. Exp. II, employing fifty-five children, aged 60–71 months, inquired into the nature of the relational patterns operating in the other two experiments.

The 'critical' pattern was found to have two components. The first was relative and resembled a qualitative size-relation more than a quantitative size-ratio. The second was absolute and involved properties of the training pair, as a pair, rather than of individual members of the pair. Practice, up to thirty post-fixation trials, emphasized the relative component, so that transposition occurred with testing pairs increasingly dissimilar to the training pair. Studies by others indicate that further practice might have led to a utilization of absolute properties of the positive member of the training pair and a consequent breakdown in transposition.

As a group, the children showed greater readiness to select the larger circle than to select the smaller. This led, during training, to slower learning when the smaller circle was positive and, during testing, to choosing the larger circle in those critical trials where transposition failed to occur.

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HAPPINESS AND UNHAPPINESS IN THE CHILDHOOD AND ADOLESCENCE OF A GROUP OF WOMEN STUDENTS: A COMPARATIVE STUDY OF ENGLISH AND AMERICAN GIRLS

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I. INTRODUCTION

In the June 1948, issue of this *Journal*, Dr W. D. Wall⁽¹⁾ published a study on 'Happiness and unhappiness in the childhood and adolescence of a group of women students'. He selected a group as homogeneous as possible in sex, age, education and social background; the group consisted of 128 women education students of the University of Birmingham. They were given a series of four questions: (a) Were you, on the whole, happy during childhood? Yes, no: underline one. If you wish to add anything to your answer, please do so here.) (b) Which things, persons, events, and so on in your childhood caused you unhappiness or unhappiness? (c) Have you, on the whole, been happy since the age of twelve or thirteen? Yes, no: underline one.) (d) Which things, persons, events, and so on have caused you unhappiness or unhappiness since the age of twelve or thirteen?

It was emphasized that all replies should be as frank and full as possible and that they could be treated as absolutely confidential and anonymous.

The average age of the group was 19, and the range was 17 to 24, although very few were outside the limits of 18–20. All the writers were above the general average in innate intelligence and in powers of self-expression.

Dr Wall's series of questions seemed to the author suitable for a comparative study between the English group and a similar American group. The author, therefore, obtained Dr Wall's kind permission to use the questionnaire and findings for such a study. His series of questions were then given to 136 freshman girl students in the School of Education at Miami University, Oxford, Ohio. The group was similar to Dr Wall's in age, sex and education.

II. QUANTITATIVE FINDINGS

(i) *The general emotional set*

The most striking quantitative fact which emerges from analysing answers to the four questions is that at both periods of life the overwhelming majority of the women students investigated rated themselves as being, on the whole, happy. Table 1 sets forth the results derived from questions 1 and 3.

There is a slight increase (2.3%) in the numbers that rate themselves as being unhappy

after the age of 12 or 13. We can therefore assume that for most American girls of this type and background the 'teens are not a period of prolonged unhappiness or even a time at which unhappy experiences predominate. This, however, is not inconsistent with a difference in the balance of happy and unhappy experiences. By counting the mentions of stimuli to happy and unhappy emotion made by the group when writing of their childhood and adolescence, we obtain an inexact, but useful, indication of this. There are 324 instances, situations, persons or things mentioned by the group when writing of their childhood, and 466 when the 'teens are dealt with. Table 2 shows how differently these mentions are distributed in the two periods of life.

Table 1. *Percentages of the group (136 women) reporting themselves as happy or unhappy during childhood or adolescence*

Happy in childhood and adolescence	Happy in childhood, unhappy in adolescence	Unhappy in childhood, happy in adolescence	Unhappy during both periods
84.4	6.7	4.4	4.4

Table 2. *Distribution of mentions of happy and unhappy stimuli in childhood and adolescence (136 women students)*

	Total no. of mentions	Percentage happy	Percentage unhappy	Significance of difference
Childhood	324	81.5	18.5	—
Adolescence	466	73.6	26.4	$P < 0.01$

Happy memories of childhood outweigh the unhappy ones by four to one, while in adolescence the preponderance of happy memories to unhappy ones is three to one. It is, therefore, fair to conclude that for women of this group, the adolescent years were, on the whole, slightly more difficult, with at least transitory causes of unhappiness, than was the period of late childhood.

The results confirm with only slight differences Dr Wall's findings, namely, that the overwhelming majority rate themselves as being, on the whole, happy. The American girls consider themselves even happier in both periods than their British counterparts: 84.4% American girls, as compared to 81.6% in the British group.

When it comes to a comparison between the distribution of mentions of happy and unhappy stimuli, in both periods the British group give a higher number of mentions: childhood, 416; adolescence 581, as compared with the Americans: 324 for childhood and 466 for adolescence. The distribution of happy and unhappy stimuli between the groups of girls is, however, different. On the whole, the American girls have many more happy memories than their British counterparts: childhood, 81.5%, as compared with 68.4% and 73.6% in adolescence as compared with only 53.3%. On the other hand, the number of unhappy stimuli is much less among the American girls: 18.5% in childhood as compared with 31.6%; and 26.4% in adolescence as compared with 46.7% mentioned by the British girls.

(ii) *Age incidence of changes*

There are indications that for some of these young women there were phases during the 'teens when they were relatively more or less happy than at others. The questionnaire did not specifically indicate this particular point, and it is, therefore, rather striking that it is only in adolescence that these broad variations are mentioned. Eleven girls stated that

or a year or two at the age of 12 they were markedly less happy; four registered 13 as change for the worse; 7 mentioned the year 14 as a change for more happiness; one, 16, and three, 17. That leaves 110 unaccounted for.

(iii) *Changes in the relative importance of the major fields of experience*

A quantitative analysis can be pushed a step further and an attempt be made to classify the mentions of incidents, persons, situations or things in the fields of experience or aspects of the psychological environment in childhood and adolescence. It should be mentioned again, as in the English research, that all these categories are interrelated and affect each other. It is, nevertheless, legitimate to separate them for study purposes. The results of this analysis are shown in Table 3.

Table 3. *The relative importance of various fields of experience in happiness and unhappiness in childhood and adolescence (136 women)*

Field of experience	Percentage of group mentioning		Distribution of mentions				Significance	
	Childhood (i)	Adolescence (ii)	Childhood		Adolescence		Columns (i) and (ii)	Columns (iii), (iv), (v) and (vi)
			Happy (iii)	Unhappy (iv)	Happy (v)	Unhappy (vi)		
Family group	85.3	69.1	49.1	10.8	24.9	10.9	Not significant	$P < 0.01$
School and university	17.6	43.4	7.1	0.3	12.2	4.1	$P < 0.001$	$P < 0.02$
Physical environment	11.0	14.0	4.3	0.3	3.6	0.6	Not significant	Not significant
Social relationships	42.6	66.9	16.0	5.9	26.8	8.6	$P < 0.02$	Not significant
Games, sports, hobbies, and interests	5.9	5.1	2.8	0.3	2.1	0.0	Not significant	Not significant
Personal future	0.0	2.2	0.0	0.0	0.0	0.6	Not significant	Not significant
Miscellaneous	5.1	14.7	2.2	0.9	3.0	2.1	$P < 0.01$	Not significant

The figures of columns childhood (i) and adolescence (ii) show in the American girls change in the relative importance of certain fields of experience. One fact is obvious: the decrease in the importance of home and family in adolescence. At this age there is growing responsiveness to wider social groupings. Another interesting fact evident in the American group is the greater emphasis on school and university in adolescence: 17.6% in childhood, but 43.4% in adolescence. But this emphasis on school and university cannot compare with an emphasis on another social grouping, namely, social relationships. Social relationships have already been very important during the childhood of the American girls. The percentage of mentionings was 42.6% in childhood, but in adolescence the mentionings are increased to 66.9%. For the American girl social relationships seem to be nearly as important as family life. A very slight increase can also be detected in the growing awareness of the physical environment. Another category that shows an increase in adolescence is the miscellaneous category. Those miscellaneous mentions mainly indicate intensified emotionality and increased introspectiveness.¹ Some references are made to fear of death, but only a few. Most of all, religious experiences play an important role here. Few girls mention games, sports, hobbies and interests in adolescence. Very few, if any, mention a special interest in the fine arts, in literature (no special book or author was mentioned in any of the questionnaires). Music, however, is frequently mentioned as an interest.

The figures of columns (iii), (iv), (v) and (vi) indicate how the change between childhood

¹ Compare Dr Wall, op. cit.

and adolescence is felt. The American adolescent girl considers her childhood life with the family happier than the adolescent years; the ratio of happy to unhappy mentions is nearly five to one in childhood. But the family still provides happy experience for the American adolescent girl. The ratio of happy to unhappy mentions is two and a half to one in adolescence. The picture is the same in other fields of experience. Happy mentions prevail in all the American group, when it comes to school and university, social relationships, games, or sports, hobbies and interests. The only exception is a column on the personal future, but its mentions are statistically unimportant.

The difference between the two groups of girls, English and American, becomes more obvious when one compares the relative importance of various fields of experience in happiness and unhappiness in childhood and adolescence. Family life, school and university, physical environment, social relationships, games, sports, hopes and interests, personal future and miscellaneous factors are bound to show the influence of the cultural pattern under which the girls grow up. Detailed comparisons between the findings of Dr Wall and his American counterpart will corroborate this.

Family life, although it declines in importance as seen by the girls in both groups, has still more mentions by the American girls than the British: 85.3% of the American girls mentioned the family as important for happiness and unhappiness in childhood, as compared with 76.6% of the British. In both groups the percentage of mentions of the family decreases in adolescence, since both groups experience new sources of happiness and unhappiness during this period. The American girls, however, still refer to the family in 69.1% of their mentionings, as compared to only 44.5% of the British.

The number of mentions for school and university shows the influence of culture more clearly. The British girls, apparently, take school and university as an important field of experience: 46.9% mention this field of experience in childhood and 65.6% in adolescence. School and university does not seem as important to the American girl; the corresponding number of mentions for the American girls is 17.6% for childhood and 43.4% for adolescence.

Another important difference between the two groups can be seen in the number of mentions concerning games, sports, hopes and interests. The percentage of group mentions for the British girls is 17.2% for childhood and 23.4% for adolescence. The number of mentions in the American group is meagre; 5.9% in childhood and only 5.1% in adolescence.

Another field of experience which indicates a difference between the groups can be seen in the percentage of group mentions regarding personal future. The corresponding figures are 13.3% group mentions by the British and only 2.2% by the American girls.

III. QUALITATIVE RESULTS

(i) *Family group*

In Table 4 are shown the percentages of the group of 136 women who mention various subdivisions of experience in the family group. As already indicated, in discussing Table 3, the 'teens are marked by a decrease in the relative importance of the home and the family. The figures of Table 4 throw a light on the subtle redistribution of emotional emphasis within the family environment.

Happy relations with the parents decrease slightly in adolescence, from 39.7% in

childhood to 36.0% in adolescence. On the other hand, the unhappy relations which comprise 7.4% in childhood increase to 12.5% in adolescence. The same is true when it comes to the mentions of the home; in childhood there were 24.2% happy mentions, and 0 unhappy mentions to match. But in adolescence this figure decreases to 18.4% with 2.9% unhappy mentions to match. What accounts for some of the mentions of unhappi-

Table 4. *Percentages of the group referring to particular aspects of family group (136 women students)*

	Home in general	Parents	Siblings Childhood	Holidays, treats, toys	Relatives	Freedom from restraint	Death illness
Happiness	24.2	39.7	11.8	11.0	7.4	2.9	0.0
Unhappiness	0.0	7.4	2.9	0.0	0.0	0.0	6.6
Adolescence							
Happiness	18.4	36.0	10.3	2.9	2.9	2.2	0.0
Unhappiness	2.9	12.5	1.5	0.0	0.7	0.0	9.6

ness? Mainly conflict with one or both parents or a rebellion against restrictions of freedom. But there are other unhappy experiences also which the American girls take to heart, disagreements among the parents for one, divorce another one. Just what this family disagreement means might be best illustrated by some quotations.

A girl (18 years 3 months) wrote:

My parents were divorced. There was constant bickering, hurt feelings and quarrelling in the family. I loved my father very dearly, but I can never feel close to my mother. I am happier here at Miami University than I ever remember being at home. I hate to go home. It always means another feud with other.

A girl (18 years 5 months):

There was before college age a conflict between parents and myself about many things which made me unhappy at home. Also, my parents did not get along and arguments and fights were continuous. Frequently I found myself in the middle of their quarrels.

A girl (18 years 11 months):

I think the thing that caused me the most unhappiness was the fact that at times I felt unwanted. I was usually trying to be noticed. This was due to the fact that my family was quite active socially and were quite strict with me. I resented even more the things they forbade me to do. They became very strict with me and insisted on more than usual requirements of church. I resented this very much.

Accidents, violent death, and drinking also play an important part in individual cases. In childhood, siblings seem to be important. However, the numbers who mention their brothers and sisters during adolescence decline.

The war has separated the members of the family frequently: brothers had to go to war. The following quotations illustrate the result.

A girl (age 19 years 2 months) wrote:

My brother (two years older than I) was seeing action in the war and this caused a great strain and worry. A year after the divorce, my parents remarried and a week later my brother was sent home, safe and sound, discharged. So everything was back to normal and since then I have been completely happy.

A girl (age 18 years 6 months):

I have had a very pleasant life with my family. One of the things that disturbed me greatly was when my brother joined the Air Corps. But that was because our family had always been very close and we all missed him so much.

Toys, treats and holidays do not apparently play a very important part in the life of the

American girls. There is an obvious decline in mentions: 11.0% in childhood and only 2.9% in adolescence.

The author feels that there is more similarity than differences in the family life on both sides of the Atlantic. The experiences of the girls as to parental disagreements, misunderstanding, and divorces exist in England as well as the United States. English and American girls alike are becoming more and more critical during adolescence and articulate toward their parents.

(ii) *School and university*

Table 5. *Percentages of the group referring to particular aspects of school and university life (136 women students)*

	School and university in general	Staff	Academic study	Discipline	Examinations
		Childhood			
Happiness	8.8	4.3	0.0	0.0	0.0
Unhappiness	0.0	0.7	0.0	0.0	0.0
		Adolescence			
Happiness	28.7	2.2	5.1	0.0	0.0
Unhappiness	8.1	2.9	2.2	0.0	0.0

Most of the American girls, according to Table 5, enjoyed their school days. School does not seem to be a source of conflicts. The typical American response reads like this: 'My experience in school was happy and I enjoyed it. I had good teachers and I liked them all.'

The figures in Table 5 indicate that during adolescence the realm of experience connected with school and university gains in importance. During childhood 8.8% mention happy experiences in school, 28.7% mention the same experiences in adolescence. This is a large increase. Even larger in proportion is the number who mention unhappiness in school and university during adolescence: 8.1%. The number who indicate special aspects of school and university during adolescence also changes. In childhood academic studies are not mentioned at all. In adolescence the numbers are also small, 5.1% mention happy, 2.2% mention unhappy experiences. But these small numbers indicate at least an awareness of the importance of academic studies among some American girls. The role of the teacher also changes. Happy experiences with teachers decrease, unhappy increase. Perhaps one may interpret this fact as an indication of the more critical attitude of adolescents toward teachers. Teachers represent authority and the adolescent is hostile to authority frequently. An interesting sidelight may be thrown here on some American girls' attitude towards college. Some dislike college as such, some dislike their professors, others worry about the teaching profession which they chose as a career. But those mentions are small in number. The American girl, on the whole, seems to attach little significance to school and university. This fact can also be detected in the lack of mentions connected with discipline or examinations. On the other hand, the American girl appreciates her position among her classmates. A comparatively large number of girls mention their outstanding position in their school. Here are some typical examples:

'I graduated with honours from high school.' 'I was happy because I was the valedictorian.' 'I was president of my class.' 'I was elected queen.'

'I was unhappy my senior year in school because I was new in a small school that did not welcome strangers. I had never known people like that before and I could not take their unkind remarks and actions very well.'

A difference in culture pattern between the British and American groups becomes much more obvious by comparing the numerical values of both tables. Of the English girls 8.2% mention school and university in general as a source of happiness; of their American counterparts the percentage is only 8.8%. The percentage of the group referring to school and university as a source of unhappiness in childhood is 7.8% for the English; their American counterparts make no mention at all of unhappy experiences in school during childhood.

School and university become more important for both groups in adolescence; 35.9% of the British mentioned school and university as a source of happiness in adolescence, 10.9% as a source of unhappiness. As already shown, the American girls, also, take school and university more seriously in adolescence, but the percentages are smaller.

The different approach to school and university can also be seen by comparing particular aspects of school and university life. Academic studies, for instance: of the British girls 4.7% mention academic studies as a source of happiness in childhood and 1.2% in adolescence; academic studies as a source of unhappiness for 1.6% in childhood and 10% in adolescence. The American girls differ very much in this respect. No mention of either happiness or unhappiness in regard to academic studies in childhood are reported whatsoever. In adolescence academic studies gain in importance: 5.1% mention academic studies as a source of happiness and 2.2% as a source of unhappiness.

Another difference between the American and the English girls' evaluation of school and university may be detected in their different considerations of discipline as a source of happiness and unhappiness in school and university. Not one American girl mentions discipline either in childhood or adolescence as a source of happiness or unhappiness.

The same is true when it comes to examinations, which are not mentioned at all by any American answers to the questionnaire.

(iii) *Physical environment*

The English girls in Dr Wall's study experienced in their 'teens a change of environment brought about by evacuation to places of safety during the war. The American girls, far away from the actual dangers of war, missed that experience and the happiness or unhappiness connected with it.

The 136 women students of the School of Education at Miami University came, with few exceptions, from the rural areas and the small towns of the middle west. A few came from the eastern seaboard. Very few had travelled much. Is it the sameness of the scenery in the middle west, the lack of dramatic landscape; cornfield, meadows, small towns, which explain the paucity of expression? Those few who travelled mention this change in the physical environment, e.g. 'trips to Colorado and various other places in the United States and Canada made me very happy. I love travelling.' Another girl writes: 'I was four years of age when I took my first trip to Florida. Since then I have travelled extensively all over the United States. I have also taken trips abroad.' These are exceptions. The majority seem to take the customary environment for granted and do not comment at all.

(iv) *Social relationships*

As mentioned before, social relationships are of great importance to the American girls. This can be seen already in the childhood mentionings. In adolescence the mentions of social relationships are only surpassed by the mentions of the family group. The percentage

of group mentions of social relations in childhood are 42.6%, and they increase in adolescence to 66.9%. The figure for family mentions that was 85.3% in childhood decreases to 69.1% in adolescence. Social relationships seem to be the paramount field of experience for American adolescents. This becomes evident in analyzing Table 6. Entries

Table 6. *Percentages of the group referring to particular aspects of social relationships (136 women students)*

	Friends	Loneliness	Social contacts	Love affairs	Special aspects of the self
		Childhood			
Happiness	27.4	0.0	4.3	0.0	0.0
Unhappiness	0.0	4.0	0.0	0.0	6.6
		Adolescence			
Happiness	33.1	0.0	22.8	17.6	0.0
Unhappiness	1.5	1.5	4.3	2.9	11.8

under every heading increase here: friends, social contacts and love affairs. References to friends which comprise 27.4% in childhood increase to 33.1% in adolescence. Love affairs account for 17.6% in adolescence, and the increased awareness of the problem which social relationships present can also be seen in the increase in unhappy mentions.

There is considerable frankness in the discussion of problems of social relationships.

A girl (18 years 7 months) writes:

I was in a good sorority, seemingly; but I could neither be like the girls in the group nor did I want to be. For a while it made me unhappy, but gradually I let things take their natural course and it all worked out... My very true friends have made me happy, but a few friends have disappointed me and I feel terrible about that.

Another girl (18 years $\frac{1}{2}$ month):

I was very happy when our high school elected me their queen of that year.

Another girl (19 years 1 month):

These things caused me happiness: the activities in school, Girls Athletic Association, Dramatics Club, my change of school in the eighth grade, the boy I have dated since the tenth grade, the group of friends I associated with in high school. Living in a small community of 3000 has contributed largely to all my happiness. Since I went to a small school, I was given a better chance to express myself.

Another girl (18 years 9 months):

I had a feeling of inferiority to my friends, particularly the girls. I could not mix well with people my own age. I am learning to do this now and I think it is making me better adjusted. I think my job this summer helped a great deal. Since I have started dating I feel more sure of myself.

Social relations in adolescence consist mainly of relationships with contemporaries. Among these, members of the other sex predominate in the American scene. Girl friends are seldom, if ever, mentioned. A special aspect of the boy-girl relation in adolescence is frequently referred to as 'dating'. This typically American form of social relation starts at an early age. As one girl mentioned:

When I was 14 I began to notice the boys and they also noticed me. There were dances to go to, and I was growing up, I could wear different and nice clothes.

There are, however, certain difficulties connected with dating sometimes. Parents have different ideas: Whom to date, for example:

My parents forbade me to date Catholic boys or boys that were other than Protestant. I like a Jew!

very much and when they told me that I could not see him I was very broken up. We still see each other secretly and my family has me practically engaged to a boy that I dislike intensely.

Another girl writes:

I have had it very nice. The only real difficulty I ever had was when I started dating. Mother has never wanted me to really grow up. I am having a lot of difficulty in that right now and I wish she weren't like that.

'Dating' is considered a source of happiness, as one girl (18 years 6 months) states briefly of happiness in childhood, 'A very happy family life'. And for adolescence, 'Same as above plus college and my boy friend'.

Some adolescents are aware of differences in their social adjustments. As one girl expresses it:

Everyone tells me I am not like the other girls of my age. I feel screwed up inside with a group of unfamiliar people or strangers.

The differences between the two groups of girls are less striking than in the preceding items. There are, however, certain slight differences which may be associated with the difference in the social scene. The American girl, a very gregarious type, enjoys social contacts and seeks them. She doesn't want to be lonely, and associates more readily with other people than her British counterpart. This can be seen by comparing the columns 'social contacts' as a source of experience: among the British it is 10.2%; among the American girls it is more than double 22.8%.

Self-awareness as a source of happiness or unhappiness, although not unknown to the American girl, is less important: no American girl mentions it as a source of happiness, 1.8% as a source of unhappiness.

(v) *Games, sports, hobbies and interests*

There is little evidence of any importance attributed by the American girls to any of the topics mentioned in this column. Reading as a source of interest is very seldom mentioned, nor is the theatre, nor the movies, nor listening to the radio, nor going to museum. These cultural activities, with the exception of the appreciation of music, do not seem to enter at all into the American girl's picture of life. It may be that the small towns and rural communities where they spent their childhood and adolescence have failed to develop their interest in these areas. It may be that theatres and museums were beyond their reach. What about movies? Radio? Books? The author has no answer for this lack of intellectual interest in a group of college students. Perhaps during their college years the girls may develop an interest in these spheres which they lack in their freshman year.

The only interest which is mentioned frequently is music. Sometimes, however, it is coupled strangely with another interest with which it has very little in common: sports.

A girl (18 years 6 months) writes:

I enjoyed sports and I got pleasure from my piano playing.

Another girl (18 years 6 months):

I got a bicycle and a piano when I was 12. This made me very happy, especially the piano.

This particular girl (18 years 8 months) seems to be an exception:

Things which cause me happiness are to listen to classical music, long walks in the woods, gardening, and fellowship with people. Also a source of happiness is my four sisters, as I am a person who likes people around me. Reading is another source of great enjoyment and I read whatever and whenever I can.

(vi) *Personal future*

Very few girls mention their personal future at all, and if they do mention it, it is only that they intend to marry their present boy friend. Not one of the girls mentions the future career that they had chosen when they came to college.

(vii) *Miscellaneous*

Among the miscellaneous experiences which are mentioned by the American girls, religion plays the most important part. Typical utterances are:

There was great religious controversy in the school and town and therefore constant fighting about religion in public schools.

Another girl writes:

After we moved to this different neighbourhood, several miles from our other home, we neglected to go to church, which has brought me much unhappiness.

There is sometimes (not often) an awareness of the self. One girl writes:

When I am unhappy, it is because of death or illness of someone close or of something sad which has happened, and also when I am feeling sorry for myself.

Another girl (18 years 7 months) says:

I have been happy most of the time. My friends and parents have made me happy. I get in moods and feel unhappy only temporarily and from unimportant incidents. Also, now and then I feel left out of things and get a slight feeling of inferiority.

A girl (19 years 7 months) reports:

I could never forgive myself for being me. When I tried to change, people wouldn't let me or help me.

Another (18 years 3 months) writes:

I have been happy as I have learned to stand on my own two feet. I have wonderful and understanding parents, and I have been fortunate in living in a good home and attending a nice high school. I sometimes have a yearning, though, for something I cannot explain.

IV. CONCLUSIONS

Dr Wall stated that his data in their quantitative and qualitative aspects 'vividly illuminate two main periods of development, childhood and adolescence'. He also said that the period of adolescence cannot be regarded as distinct from childhood, but as 'emergent from it, a deepening and widening, a repatterning of the emotional field'. The American data presented in this paper support these findings.

However, these American data do not reflect in quite the same way the two major trends of development which Dr Wall found interacting with each other and 'operating to deepen and enrich the personality'. The author did not find enough evidence among the American girls of the first of these which Dr Wall calls the 'growth of self-awareness'. The American girl, it seems to the author, has not at this stage reached a conscious conception of herself. Apparently she has little interest in introspection, instead enjoys social life, the experience of meeting new people, and does not find an interest in looking into herself.

The second trend of development which Dr Wall calls 'a product of what parents, society and contemporaries expect of the adolescent' is, however, very obvious in the American group. The British girls responded to their environmental and cultural conditions; the American group, too, responded to their particular environmental and cultural conditions. Differences in the two groups can nearly always be explained by differences in the social scene; English society expects something else in some areas from the British girls than American society does from the American girl. This can be seen most clearly in the areas of school and university, social life, hobbies and interests.

Dr Wall's point of departure was to find out if adolescents, as many writers from Stanley Hall onwards have contended, is a period of storm and stress. From the English data he concluded that there is no evidence that girls of this intellectual and social level regard the 'teens as predominantly unhappy. Even more than the English data do the American answers to the questionnaires underline this fact.

V. SUMMARY

1. 136 young women between the ages of 17+ and 22 in the School of Education of Miami University, Oxford, Ohio, as compared with 128 young women between the age of 17+ and 22 in the Teacher-Training Department of the University of Birmingham, England, were asked to complete a schedule on the incidence and causes of happiness and unhappiness in childhood and adolescence.

2. 84.4% of the American group rated themselves as happy both in childhood and adolescence, 4.4% were unhappy in childhood, 6.7% in adolescence.

The corresponding figures for the British group are: 81.6% rated themselves as happy both in childhood and adolescence. Less than 8% were unhappy in childhood, less than 2% in adolescence.

3. With the American girls happy memories of childhood outweigh the unhappy ones four to one; while in adolescence the preponderance of happy memories to unhappy ones is three to one.

The corresponding findings of the British group indicate that, whereas two-thirds of the recollections of childhood are happy ones, only just over half of those from the 'teens are happy. It is, therefore, fair to conclude that for both groups the 'teens show an increasing emotional stress.

4. In the English and in the American group alike, the importance of the home and family declines in the 'teens and wider social experiences become more significant. School and university as fields of experience, however, mean more to the British girls than to their American counterparts. In the references to the family there are on both sides of the Atlantic more mentions in the 'teens of clashes with the parents, of disagreements with the parents' ideas. The British group seems to be more aware of conflicts than the American group, however.

5. In general social relationships the picture is very much alike in its general trend, though the American girls enjoy the social experiences more than the British girls and are less aware of social shortcomings.

6. Games, sports and hobbies and intellectual interests seem to offer more attraction to the British girls. The American girls, under the influence of a different culture, do not seem to have discovered this area at this point.

7. There is no evidence in either group to suggest that storm and stress is a necessary accompaniment of development in the 'teens, nor does it seem to be a product of the culture of either nation.

REFERENCE

- (1) WALL, W. D. (1948). Happiness and unhappiness in the childhood and adolescence of a group of women students. *Brit. J. Psychol.* xxxviii, 191-208.

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SOME CORRELATES OF OPINIONS ON THE UPBRINGING OF CHILDREN

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. *Aims of the investigation* (p. 141). II. *Instruments of measurement* (pp. 141-144): (i) *Parental opinion inventory* (pp. 141-142); (ii) *Social attitude inventory* (pp. 142-144); (iii) *Word Connexion List* (p. 144). III. *Collection of the data* (p. 144). IV. *Analysis of the data* (p. 144). V. *Outcome of the analysis* (pp. 144-145): (i) *Main results* (pp. 144-145); (ii) *Consistency of heterogeneity* (pp. 145-146); (iii) *Deviation of regression from linearity* (pp. 146-147). VI. *Discussion and conclusion* (pp. 147-149). *References* (p. 149).

I. AIMS OF THE INVESTIGATION

It is the routine practice of child-guidance clinics to interview their patients' parents. One of the main purposes of such interviews is to elicit attitudes on the upbringing of children. The kind of question which is asked and the way in which the answers are interpreted will depend on many factors. Among them will be the clinician's theories about child development and his ideas of the way in which social and personal factors enter into the parent's answers.

Neither area of theory can be said to have been validated, and the assumptions which operate in practice vary considerably from clinician to clinician.

It was with such problems in mind that it was decided to carry out a preliminary investigation of opinions on the upbringing of children. This investigation had two main purposes: first, to find out how such opinions are related to opinions on social matters, and secondly, to find out how they are related to temperament.

II. INSTRUMENTS OF MEASUREMENT

Three tools were used in this investigation. These were a specially devised inventory of opinions on the upbringing of children, Eysenck's social attitude questionnaire (2), and the Crown Word Connexion List (1).

(i) *Parental opinion inventory*

Theoretical basis. Current theories of child development are concerned, among other things, with the influence of parent-child relationships. Each theory emphasizes certain kinds of parent-child interaction as being basic. The remaining situations are either ignored or it is insisted that they are dependent on the way in which allegedly more fundamental situations have developed.

A priori, there is no particular reason why one kind of situation should be more important than another. It is clear, of course, that the child in the course of his growth will pass through changing patterns of relationships with his parents, and with a little imagination it is possible to elaborate any one or more of these situations into a new theory of personality development.

It was decided to make the items of the questionnaire reflect two theories of child development: an 'Analytical' type of theory and a 'Restrictive' theory. It was assumed that the first would deal with anal, oral, sexual and sibling-rivalry situations. Examples

of the kind of item developed are no. 3: 'A mother should try to get her baby to use the pot from birth', and no. 11: 'My husband and I never kiss one another in front of the children'. Such items are indicated by the letter A in Table 1.

The second group of items, belonging to the 'Restrictive' theory, deal with parental reactions to the free expression of a child's desires, whatever they may be. Items dealing with a variety of situations, such as social manners, choice of friends, mode of dress, punishment for misdemeanours, were therefore chosen. These items will be referred to as 'Restrictive' items and are indicated by the letter S in Table 1. Examples of this type of item are no. 34: 'A child should be free to handle as many things as possible in the house', and no. 25: 'A child should have as much say as it is capable of in family affairs.'

Further investigation showed that many items appear to be of mixed 'Analytical' and 'Restrictive' content. Examples are no. 17: 'I don't mind if my child is jealous of its baby brother or sister', or no. 9: 'I told my child the facts when he asked where babies came from.' It was decided to include such items. They will be referred to as the 'Mixed' group and are indicated by the letter M in Table 1.

In order to increase the precision of the results, it was decided to have a fourth group of items which, in the judgement of the writer, contained neither 'Analytical' nor 'Restrictive' elements, but which might have an important bearing on parent-child relations. These items were called 'Neutral' and are indicated by the letter L in Table 1. Examples are no. 40: 'Women marry more for a home than for love', and no. 33: 'Fathers should leave the mothers to bring up the children.'

Development of the inventory. The main method used in the construction of the inventory was to have the items, which had first been formulated on the theoretical grounds given above, answered by mothers of varying age and social status and to discuss the items with them from the point of view of both their content and formulation. In all, over fifty mothers participated in this work, and the final result is the opinion inventory given in Table 1.

No attempt was made to keep the number of items in each category equal. The discussions with the mothers decided the choice of the items, the only qualifying factor being the apparent power of an item to evoke a sufficiently wide range of responses from a group of normal mothers. Any item or formulation of an item which appeared to evoke a unanimous reaction among the mothers was rejected. In this way, 40 items finally emerged, 18 of which were 'Restrictive', 4 'Analytical', 9 'Mixed' and 9 'Neutral'.

The opening instructions, which can be read in Table 1, were also a result of this discussion. This applies especially to the injunction to judge the statements in 'the light of what you think is the right thing to do rather than what you actually do yourself'.

(ii) *Social attitude inventory*

Eysenck's Social Attitude Inventory was given. This consists of forty attitude statements on social and political matters.

One of them is a measure of Radicalism. This is posited by Eysenck as one of the two main dimensions in the structure of social attitudes. It is characterized at one extreme by such attitudes, to quote Eysenck(3), as 'Nationalization is inefficient', 'General strikes should be made illegal', 'My country right or wrong', 'Unemployment is the only incentive which will make people work'. The other extreme is characterized by attitudes such as: 'Private property should be abolished', 'The laws favour the rich', 'Give up

Table 1. *Parent opinion inventory*a) *Initial instructions given to subject before beginning the inventory.*

Look below. You will find a number of statements made by mothers about their children. For example, the first one reads: 'I want my child always to be neat and clean.'

Underneath each statement you will see these phrases:

Strongly agree Mildly agree Uncertain Mildly disagree Strongly disagree

We want you to draw a line, in pencil or in ink, under the phrase which expresses your feelings best about the statement made above it.

We want you to judge these statements in the light of what you think is the right thing to do rather than what you actually do yourself.

b) *Inventory items with a priori classification and F ratios*

KEY: R indicates Radicalism
T indicates Tendermindedness
N indicates Neuroticism
S indicates Restrictive items
A indicates Analytical items
M indicates Mixed items
L indicates Neutral items
* indicates F significant 5% 1%
** indicates F significant 1% 0%
*** indicates F significant 0.01%

Item no.	Classification	Content	R	T	N
1	S	I want my child always to be neat and clean.	*	*	.
2	S	I think that punishment does no good to children.	***	.	*
3	A	A mother should try to get her baby to use the pot from birth.	.	.	.
4	A	It is harmful for a two-year-old child to share its parents' bedroom.	.	.	.
5	S	I am very strict with my child.	**	.	.
6	L	I try to spend as much time as possible with my child.	*	.	.
7	L	It is the father, not the mother, who should punish the child.	**	.	.
8	S	I always keep my child away from dangers.	.	.	.
9	M	I told my child the facts when he asked where babies came from.	.	.	.
10	S	I sometimes slap my child when it does something wrong.	*	.	.
11	A	My husband and I never kiss one another in front of the children.	.	*	.
12	M	It's no use training the child to use the pot until it can understand what you tell it to do.	.	.	.
13	S	When my child does something wrong I sometimes say 'You are a bad child'.	.	.	.
14	N	What was good enough for me should be good enough for my children.	.	.	.
15	A	A child needs to be kissed, hugged and fondled.	.	.	.
16	S	I don't think you should let a child cry ever.	.	.	.
17	M	I don't mind if my child is jealous of its baby brother or sister.	*	.	.
18	S	I let my child have any children he likes for his friends.	***	.	.
19	S	Children should always obey their parents without question.	***	.	.
20	M	It is best to leave a child to wean itself from the bottle in its own good time.	.	**	.
21	S	When my child does something wrong I sometimes take away something I have given it	.	.	.
22	L	I expect my child to be grateful for all its parents have done for it.	***	.	.
23	M	When my child was a baby I always milk-fed it when it cried in the middle of the night.	.	*	**
24	L	A father can't understand 'mother love'.	.	*	.
25	S	A child should have as much say as it is capable of in family affairs.	*	.	.
26	M	Babies should be fed strictly by the clock.	.	.	.
27	M	It doesn't matter if a boy prefers his mother and a girl her father.	.	.	.
28	S	I believe in the saying, 'spare the rod and spoil the child'.	*	**	.
29	M	When my child does something wrong I sometimes say 'Your brother never does that kind of thing'.	.	.	.
30	L	Children should find out the facts of life from their teachers and not their parents.	.	.	*
31	S	I let my child bring any children he likes into the house, even ones I don't like.	***	.	.
32	S	I let my child learn by his own experience, even if he has to risk hurting himself.	.	.	.
33	L	Fathers should leave the mothers to bring up the children.	.	.	.
34	S	A child should be free to handle as many things as possible in the house.	***	.	.
35	M	When my child was a baby I rocked him to sleep when nothing else would quieten him.	.	.	.
36	L	I sometimes get completely fed up with my child.	.	*	.
37	S	When my child was four years old I refused to give it anything if it did not say 'Please'.	.	.	.
38	S	If a four-year-old child interrupts when grown-ups are talking, he should be silenced firmly.	**	.	.
39	S	Sometimes when my child does something wrong I send it out of the room.	.	†	*
40	L	Women marry more for a home than for love.	.	*	*

national sovereignty to preserve peace' (3). The score on Radicalism correlates with voting behaviour and can be regarded as measuring at least an important part of what is usually regarded as political opinion.

The second measure is that of Eysenck's second factor, Tendermindedness (3), which is orthogonal to Radicalism. This is defined, at one extreme, 'by materialistic, hedonistic, toughminded attitudes, such as those calling for euthanasia, compulsory sterilization, and the abolition of laws against abortion, as well as attitudes of superiority towards women, Jews, and coloured people, and an approval of war, compulsory military training, flogging and capital punishment. At the other end we have idealistic tenderminded attitudes such as those approving of church going and religion, pacifism, the curing, rather than the punishment, of criminals, and the equality of the sexes and races' (3).

(iii) *Word Connexion List*

The Crown Word Connexion List was used. This is a forced-choice word association test, in which the score is the number of neurotic response words chosen. Its validity, according to Crown (1), is not high enough for individual psychological work, though it is considered sufficient for the indication of trends in a preliminary large-scale investigation.

III. COLLECTION OF THE DATA

Completed records were obtained from 197 subjects for the Parental Attitude Inventory, Social Attitude Questionnaire and Word Connexion List. These people were partly of working class and partly of middle class status. They could not be regarded as a representative sample as they were the contacts of psychology students in University extension courses. Of these 41 % were male and 59 % female. Their average age was 35 years with a standard deviation of 8 years. Of the sample 45 % had children with a mean size of family of two children.

IV. ANALYSIS OF THE DATA

The first step taken in the examination of the data was the analysis of the relationship between the items of the Parent Opinion Inventory and the three variables: Radicalism, Tendermindedness and Neuroticism. As linearity in these relationships could not be assumed, analyses of variance were carried out between each item in the Parent Opinion Inventory and the three other variables. Thus 120 analyses were done. The level of significance of each ratio is given in Table 1 in terms of the variance ratio F . In order to have an indication of the extent of correlation, epsilons¹ were calculated for those items which produced F 's significant at the 5 % level. These are given in Table 3.

V. OUTCOME OF THE ANALYSIS

(i) *Main results*

The main conclusions from the data are represented in Tables 1 and 2. In general it is clear that Radicalism, Tendermindedness and Neuroticism all provide significant relationships with the opinions on the upbringing of children. Of these Radicalism, with 14 significant F 's out of a possible 40, was the most potent, while Tendermindedness had

¹ Epsilon is described by Peters & Van Voorhis (4) as a correction of a correlation ratio which is said, without this correction, to be biased by difference in the number of items in the several classes.

7 out of 40 and Neuroticism had 5 out of 40. The multiple correlation coefficient between the nine items giving F 's at a 1% level of significance and Radicalism was 0.525.

Looking at the F ratios from the point of view of the parent opinion items, we find that all the four groups provide significant F 's. The Restrictive item, with 15 significant F 's out of a possible 54, and the Neutral item, with 8 significant F 's out of a possible 27, account for most of the significant results. The mixed items provide only two cases out of a possible 27, where the means differ at the 5% level and the 'Analytical' 1 out of a possible 12.

Table 2. *Distribution of significant F 's*

Item classification	Total possible F 's per cell	Variables			Total significant F 's per item classification	Total possible F 's per item classification	
		Radicalism	Tender-mindedness	Neuroticism			
Restrictive	18	1% level	7	1	0	8	54
		5% level	4	1	2	7	
Analytical	4	1% level	0	0	0	0	12
		5% level	0	1	0	1	
Mixed	9	1% level	0	1	1	2	27
		5% level	0	0	0	0	
Neutral	9	1% level	2	0	0	2	27
		5% level	1	3	2	6	
Total significant F 's per variable		14	7	5	26	—	
Total possible per variable		40	40	40	—	120	

Example of interpretation of table: There was a total of 18 'Restrictive'-Radical combinations, of which 7 were at <1% level of confidence and 4 between the 5 and 1% level. Similar readings can be made for each combination: 'Analytical'-Radical, and so on.

An examination of Table 2, which breaks up the results given in Table 1, results in the emergence of some definite generalizations. First there appears a strong relationship between Radicalism and the 'Restrictive' items. There is a total of 11 significant F 's out of a possible 18. Of these 11, 7 are significant at the 1% level. An examination of the direction of the relationships shows that, in every case, the more 'restrictive' the subjects are, the lower their scores on Radicalism.

The only other grouping which can compare with the Radical-'Restrictive' grouping is the Radical-'Neutral' one. This gives 3 significant F 's out of a possible 9, with 2 of the 3 significant at above the 1% level. Only further analysis can show what light these results throw on the structure of parental opinions.

A second generalization which emerges is that there is a complete absence of significant F 's for the Radical-'Analytical' and the Radical-'Mixed' groupings. This would imply that, whereas items involving restrictions alone have a strong relationship with political opinions, those items involving 'Analytical' situations have no relationship at all with such opinions. This applies even when the items have an element of restrictionism in them, as the 'mixed' groups do.

For the rest, the picture is featureless. The remaining significant F 's with Neuroticism and Tendermindedness are distributed evenly and sparsely among the remaining eight combinations shown in Table 2.

(ii) *Consistency of heterogeneity*

A second feature of the results is that the variance in 'Restrictive'-Radical analyses show a consistent heterogeneity. Subjects at the restrictive end of the scale show far less variance in their political opinions than those at the tolerant end.

This implies that one would predict with greater accuracy the Radicalism score of a restrictive subject than that of a tolerant subject. A similar finding has been reported by Eysenck(2).

(iii) Deviation of regression from linearity

The third outcome is a methodological one. An investigation was carried out to see if the relationship between the significant items and Radicalism would have been seriously distorted if linearity had in fact been assumed and the usual product-moment correlations calculated. Such correlations were therefore calculated for all the analyses which gave *F*'s significant at the 1 % level. They are given in Table 3, where they can be compared with the epsilons which were calculated without the assumption of linearity. In addition there were calculated tests of the significance of the deviations of the regressions from linearity. These too are given in Table 3.

Table 3. Epsilons and *r*'s at <1 % level of significance*

Item no.	Radicalism		Tendermindedness		Neuroticism		Non-linearity
	<i>e</i>	<i>r</i>	<i>e</i>	<i>r</i>	<i>e</i>	<i>r</i>	
2	0.327	0.039	—	—	—	—	<20 % >5 %
5	0.286	†	—	—	—	—	Missing†
7	0.264	0.117	—	—	—	—	<1 %
18	0.311	0.321	—	—	—	—	<20 % >5 %
19	0.312	0.239	—	—	—	—	<1 %
20	—	—	0.264	0.263	—	—	>20 %
22	0.397	0.396	—	—	—	—	>20 %
23	—	—	—	—	0.221	†	>20 %
28	—	—	0.226	0.086	—	—	<1 %
31	0.275	0.270	—	—	—	—	<20 % >5 %
34	0.295	0.302	—	—	—	—	>20 %
38	0.242	0.242	—	—	—	—	>20 %

Key. *e* = epsilon; *r* = product moment *r*. Non-linearity column gives significance of deviation from linearity. For content of items see Table 1.

* All *F*'s at <5 % level give epsilons between 0.117 and 0.202.

† The record of the computations for this figure has been lost. Other records indicate that they do not disturb the general trends of the finding reported in this paper.

It appears that five of the twelve items did not show a divergence from linearity above the 20 % level of significance. Linearity of regression can, in the case of these items, be safely assumed. Three of the remaining items must undoubtedly be accepted as non-linear, all being at the 1 % level. The regressions of these three items on their respective variables are illustrated in Table 4. It will be seen that in each case the non-linearity appears to be due primarily to the misplacement of an extreme approval or disapproval. As Dr Ehrenberg has pointed out, it would appear as if there were two separate monotonic functions for each regression. In item 28, for example, we have 1, 2 and 3 in proper order and 3, 4 and 5 in equally proper but reversed order. The same can be seen in item 19. Item 7 shows almost exactly this twin monotonic function with slight, but not significant, displacement of grades 3 and 4.

To generalize, it would appear that occasionally in the field of social attitudes there is a tendency for extremes to meet. The formulation of hypotheses to account for this observation would require a detailed inquiry into the characteristics of the sample participating in this investigation.

The remaining five items are doubtful, being between the 5 and 20 % levels of significance.

Examination of the correlations and epsilons in Table 3 shows that a false assumption of linearity would have distorted three of our correlations. In these three cases the product-moment r , appear to be considerably lower than the corresponding epsilons.

Table 4. *Significant deviations from linearity*

Key. □ = Number of subjects
① = Strongly agree
② = Mildly agree
③ = Uncertain
④ = Mildly disagree
⑤ = Strongly disagree

Item 28. I believe in the saying 'Spare the rod and spoil the child'.

																														15	49	60	57	16																														
																														③	②	④	⑤	①																														
20	1	2	3	4	5	6	7	8	9	30	1	2	3	4	5	6	7	8	9	40	1	2	3	4	5	6	7																																					
Tendermindedness score																																																																

Item 7. It is the father, not the mother, who should punish the child.

										51	36											12	95											3										
										④	③											②	⑤											①										
20	1	2	3	4	5	6	7	8	9	30	1	2	3	4	5	6	7	8	9	40	1	2	3	4	5	6	7																	
Radicalism score																																												

Item 19. Children should always obey their parents without question.

Figure 1 shows a horizontal scale for Radicalism score, ranging from 20 to 40. Above the scale, five boxes contain numbers: 10, 45, 68, 18, and 56. Below these boxes are circled numbers: 3, 2, 4, 1, and 5 respectively. The scale itself is marked from 20 to 40 in increments of 1, with labels 20, 30, and 40 at the major intervals.

VI. DISCUSSION AND CONCLUSIONS

The first question behind the investigation, that concerning the relationship between opinions on the upbringing of children and opinions on social matters, has received an answer. The results indicate that opinions on matters affecting the freedom of expression of a child's wishes are significantly related to a person's political opinions. Subjects in this sample who voted Labour would tend to be tolerant in their attitude to the free expression of children's desires, while those subjects who would vote Conservative would tend to be intolerant of such expression.

This finding is given additional point by the fact that the 'Restrictive' items and those which formed the Radicalism scale did not have a specific content in common. The first deal only with parent-child relationships and the second deal only with social and political matters. This difference in the specific content indicates the possibility of considerable unity in the underlying organization of attitudes with apparently different content. Such generalization is again indicated by the considerable number of significant F 's provided by Tendermindedness, though in this case an explanation will not emerge so easily as in the case of the Radical-'Restrictive' grouping.

It must be remembered, of course, that subjects were asked to think of what they *ought* to do rather than of what they actually *did* do. Another consideration which might limit the generality of the implication of these findings is that the inventories were filled in in the year 1946. We also know that the sample was heterogeneous for marriage, age, sex and parenthood. It may be possible, for instance, that the unmarried members of the sample

provided the correlation, whereas the married ones did not; and so on. These matters can only be cleared up by further investigation.

The relationship between the parental opinion items and Tendermindedness is not clear and strong as it is with Radicalism. Nevertheless, the relationship does seem to be a definite one. Seven items out of a possible forty provide F 's above the 5% level of significance, and two of these are above the 1% level. A closer examination of the significant items indicates a possible common content. Most of them seem to be suggestive of the love-hurt-hating theme (see items in Table 1). Examples are no. 11: 'My husband and I never kiss one another in front of the children', no. 24: 'A father can't understand "mother love"', no. 28: 'I believe in the saying "spare the rod and spoil the child"', and no. 36: 'I sometimes get completely fed up with my child'. To confirm this notion more items of the same kind would have to be invented and correlated with Tendermindedness.

The second question put, that of the relationship between opinions on the upbringing of children and temperament, produced a definite but unclear relationship; unclear, that is, in terms of the initial classification of items made. Five items out of forty provide F 's at the 5% level, one of them being at the 1% level. Here again an examination of the content of the relevant items is suggestive. Most of them might be said to deal with the avoidance of situations involving conflict for the parent. Examples are no. 23: 'When my child was a baby I always fed it when it cried in the middle of the night', no. 30: 'Children should find out the facts of life from their teachers and not their parents', and no. 40: 'Women marry more for a home than for love'. There are, however, many items which do not correlate significantly with Neuroticism but which would also be identified as involving conflict. Further investigations would be necessary with a more sensitive measure of Neuroticism.

A third outcome of this investigation is that the assumed theory of child development obviously affects the results of an investigation such as this. While the 'Restrictive' items were positively correlated with Radicalism, the 'Analytical' items showed no such relationship. Not one significant F with Radicalism was provided by either the 'Analytical' items or the 'Mixed' items. We must remember that the 'Mixed' items are a mixture of 'Restrictive' and 'Analytical' elements, and so it appears that, when a 'Restrictive' item has an 'Analytical' element in it, it shows no relationship whatever with Radicalism. This finding is provocative of many speculations which can only receive an answer by experiment. Such speculations would be influenced by the fact that items with some kind of 'Analytical' content provided only three significant F 's, with the remaining variables Tendermindedness and Neuroticism.

The main aim of further investigation might be to find out what relationship there is between responses to this inventory and the kind of children which the responders produce, and of special interest would be a comparison in this respect of the 'Analytical' with the 'Restrictive' items. That such an investigation would be fruitful is indicated by Shoben's work (5). He compiled a Parent Attitude Inventory of 148 items, having previously classified the items into four different groups: 'Ignoring', 'Possessive', 'Dominating' and 'Unclassified'. He tested the power of these groups to discriminate fifty parents of problem children from fifty parents of non-problem children. The 'Dominant' scale gave a point-biserial r of 0.801, the 'Possessive' scale 0.790, and the 'Ignoring' scale 0.968. He repeated the experiment with twenty non-problem and twenty problem parents and point-biserials of 0.623, 0.721 and 0.624 respectively were obtained. The first implication

these results is that the answers to questionnaires have a real relationship to the parents' treatment of their children. The second point of interest arises from an examination of the actual items used in Shoben's 'Dominating' category. Most of them are very similar to our 'Restrictive' items. In fact, many of the items are identical in content. In general conclusion, one may say that, though there are strict limitations to the conclusions which can be drawn from this investigation, especially due to the heterogeneous and unrepresentative character of the sample, certain definite results emerge. These results at least allow us to pose some definite questions about the organization and connexions of parent opinion and provide a demonstration of the possible range of influence of the primary social attitudes of Radicalism and Tendermindedness.

Acknowledgement is made to Dr H. J. Eysenck for his suggestions and advice, and for arranging for the collection of the data; to Dr A. Lubin for his help with statistical design and interpretation; to Dr A. Ehrenberg for some discussion of interpretation; and to Miss A. Davis for preparing and typing the manuscript.

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PUBLICATIONS RECENTLY RECEIVED

The Origins of European Thought about the Body, the Mind, the Soul, the World, Time and Fate. By R. B. ONIANS. London: Cambridge University Press. 1951. Pp. xvii + 544s. net.

This fine book by the Hildred Carlisle Professor of Latin, London University, is primarily a major work of classical scholarship, the fruit of nearly 30 years' research and revision. As such it could only be done full justice by a reviewer as eminent as Prof. Onians in his own field. But as he has suggested in the preface 'the work may be of some interest to... philosophers, anthropologists, historians of science and students of religion as well as to the plain man who would penetrate into pre-historic times and the beginnings of our civilization', it is not unfair for psychologists to slip in through the door and see what lies there for them. What they will find is sheer treasure to be examined quickly at first for its enthralling interest and then to be pored over and pondered.

The book's subtitle is 'New interpretations of Greek, Roman and kindred evidence also of some basic Jewish and Christian beliefs'; it is divided into three main sections entitled respectively 'The Mind and the Body', 'The Immortal Soul and the Body', 'Fate and Time'. In addition, there are some fifty pages of addenda on special topics with such tempting titles as 'Winged Words', 'Mars, Woodpecker, Woodpecker' (who could resist inquiring what links these strange book fellows?) and "'The Billie Blind" or "Belly Blind", the "Auld Belly-Blind Man"'. (Readers of the Ballads in F. J. Child's collection will recognize this last as a household spirit who appears at the bed or elsewhere to approve a marriage and attests the bride's virginity or good birth. Prof. Onians derives him from an ancestral spirit or *Lar* with a wrapping over his eyes, since the dead had such.)

The theme and variations of section I concern the translation of the Greek *φρένες* which Prof. Onians considers should be 'lungs', and the early Greek conception of *θυμός*, mind or consciousness. This is associated with the chest (through the lungs and heart), and is not merely thinking and perceiving 'breath', which departs from the individual at death, but involves an emotional element from its affiliation to the heart's blood. It took the obsessional Sage of Königsberg to separate cognition, affection and conation so ruthlessly that later psychologists began to see something amiss. If Onians restores us to our cultural roots in a conception of consciousness as physical as breath but remote from desiccated acquaintance, for this alone we should be grateful.

In section II we meet the *ψυχή*, that which is divine in origin, survives death, and is the life of the living. Prof. Onians works out in detail its early Greek association with the head, the cerebro-spinal fluid, the marrow and the seed of procreation. Around this thread he weaves a fascinating web of supporting evidence and illustration, ranging from the significance of chastity and of partial cremation (until the body was dry the *ψυχή* could not depart) through the importance of sneezing and the reason for the Pythagorean taboo on eating beans, to why the uninitiated in the world below ever vainly try to get water (while the 'holy' enjoy 'everlasting drunkenness'), why the aged might be rejuvenated by boiling and why the firm may symbolize resurrection. He explains, of course, a whole set of miraculous births from the head and other parts of the body. Of great interest to the historian of psychology should be Prof. Onians's comment on the fusion of *θυμός* and *ψυχή*, for thereby, to the reviewer, seems to hang the long European tale of increasing enrichment of the *ψυχή* with functions earlier thought to belong to *θυμός* while the original immortality of the *ψυχή* is retained (or dropped) according to the mystical or empirical persuasion of the writer.

The reference to a web brings us to section III, 'Fate and Time'. Here the unclassical reader gets more engulfed, for the detailed argument in this, more than in other sections, seems to turn on untranslated Greek. Prof. Onians deals briefly with time, illustrating the Greek interest in its qualitative aspect. His main theme is the association of fate with spinning and hence with a thread which is bound upon the person. From this and linguistic considerations he derives a new interpretation of *τέλος* as a band, bond or circle (symbolizing completion, for example, of a portion of life and so of time). Such bonds may be negative as in debt, disease or death (the 'belly' or 'bag' covering the *lar* mentioned earlier is related to this). Or the bonds may be positive as in the *τέλος* of marriage, in triumph and in the related uses of fillets, 'silken snoods' and crowns. 'Du Ring an meinem Finger' passes through one's mind. Here, if it must be critical, and a wholly uncritical review would be suspect, one feels that Prof. Onians deals a little too lightly with the reason why the Greeks, in common with their descendants, think more readily

negative than of positive occurrences as 'fated'. And here, as in all the book, one wonders a little how Prof. Onians's rival classicists will take his dismissal of their theories.

Enough has been said, however, to suggest the breadth and depth of Prof. Onians's survey, and, in spite of the liberal annotation and cross-referencing, the fact that the book is no tome of arid scholarship. In fact it is flavoured throughout with infectious curiosity and the true classical feeling for concrete and believable fact. A final word of praise should go to the C.U.P. for their usable indexes and the ingenuity of their running headlines. Only a dry soul would glance at 'Birth from the knees' or 'What was Ambrosia?' and not read on.

J. W. REEVES

Argument of Laughter. By D. H. MONRO. Melbourne University Press; London: Cambridge University Press. 1951. Pp. 264. 21s. net.

This is, in the first place, just the sort of book the earnest student asks for. It presents all the important theories concerning laughter in a clear, concise and readable way. But it is more than that. It is a book for the kind of student who wants to know not only what the theories are but also whether any of them are true. The pros and cons are argued—with acuteness, plausibility and wit.

Part I—'What we laugh at'—assembles essential facts, states the problems, draws important distinctions, isolates the central concepts. It generally collects the impedimenta for the expedition.

Part II—'Theories of humour'—works through the classical and contemporary doctrines. These are divided into *Superiority Theories*—Hobbes and Bain, Leacock and Ludovici; Bergson; Feibleman and McDougall; *Incongruity Theories*—Kant, Schopenhauer, Spencer and Eastman; *Release from Restraint Theories*—Klein, Freud and Gregory; *Ambivalence Theories*—Greig and Krishna Menon.

Part III—'Synthesis'—consists of one chapter only. The author, with due apologies, adds to the complications by presenting his own account of the matter. What it is appropriate to laugh at is not inappropriate. We do not laugh at everything that is inappropriate, but everything we laugh at is appropriate to something (other than laughter). The case is argued with plausibility. As with all the other suggestions we readily accept certain examples as fitting into the theory; but in other cases we have to be argued into accepting the interpretation offered; and when we agree (if we do) that it may perhaps be so, our agreement is reluctant.

The author, it must be disclosed, is a philosopher, and he allows himself a few philosophical foibles. He displays a truly philosophical incredulity when confronted with the facts of life. He finds it difficult to believe, for example, that our minds are connected with our bodies. 'We can hardly suppose', he says, 'that seeing someone slip on a banana skin causes any direct chemical change in the organism.' With reservations about the 'direct' (whatever this may mean) psychologists would say that it takes much less than that to produce such changes in our bodies. Mr Monro also shows the *a priori* philosopher's lack of appreciation of experimental method. 'I have not paid much attention', he confesses in his Foreword, 'to the large number of recent writers who have adopted an experimental laboratory approach to the subject. "Sense of humour tests" and the like may eventually throw light on the essential nature of humour, and some of the facts already discovered are no doubt valuable. But, so far as I know, the use of such tests has not yet resulted either in the development of a new theory or the confirmation of an old one.' But, surely to goodness, these recent writers might protest, it is no part of the experimental psychologist's ambition to *add to* the number of theories! He is quite content to make a few observations which may increase the specification of conditions to be taken into account; and in this way *reduce the number* of possible theories. Moreover, before he makes any experiment at all, the experimental psychologist asks himself: What sort of observable fact would be relevant to the theory to be tested? And this, it is to be feared, is a question which at the most crucial points fails to enter Mr Monro's mind. 'The laughable', he says, 'is a species of the inappropriate. If we go on to ask the differentia, it resides, I suggest, in this, that there is an element of appropriateness in the inappropriate, when it is funny.' The subtlety of this theory makes it the more engaging, but *how*, pray, *how* could one test the truth of this statement?

Still, criticize this book as one may, one reads on to the end with profit and with pleasure. The author's last words are delectable. 'There are still many questions to be answered... I do not know the answers.' Would anyone in the face of such disarming intellectual honesty—pervading as it does the book as a whole—do less than commend this study to colleagues, students and friends?

C.A.M.

Play, Dreams and Imitation in Childhood. By JEAN PIAGET. Translated by C. GATTEGNO and F. M. HODGSON. London: Heinemann. 1951. Pp. ix+296. 21s. net.

English psychologists must be exceedingly grateful to anyone who translates Prof. Piaget's books into English and publishes them in this country. But it is difficult to understand why this particular book was chosen, rather than *La Naissance de l'Intelligence chez l'Enfant* and *La Construction du Réel chez l'Enfant*. Both these books were written before *La Formation du Symbole chez l'Enfant*, and are on the whole more interesting and more important. Moreover, they expounded the terminology and the developmental scheme which is employed here without much further explanation. Both the terminology which Prof. Piaget uses and the argument in which he develops his hypotheses are in any case far from easy to understand; and they must present almost insuperable difficulties to the reader who has not studied either the two preceding books, or the recently translated *Psychology of Intelligence*.

It should be remarked that the difficulty lies in the original exposition, not in the translation. Prof. Piaget has presumably adopted a terminology (mainly of words of ten letters or more) to which he can assign specifically defined meanings in order to avoid the vagueness of words in common use. But the effect produced on the reader is almost that of a new foreign language which he must interpret as best he can in the light of the occasional passages written in plain English—or rather, plain French.

In his task the reader is much assisted by the delightful illustrative examples drawn from the play and conversation of Prof. Piaget's three children, Jacqueline, Lucienne and Laurent. He may well wish for more such examples to make clearer the nature of the developmental processes about which Prof. Piaget theorizes. The theory seems to run somewhat as follows: In earlier books were described the sensori-motor stages of the child's development during his first year or so, and it was shown how the child became able to identify and localize the objects and events in his immediate surroundings. But he still remained very largely centred within himself, unable to conceive the nature of the more distant environment which he could neither touch nor handle, or of the activities of beings other than himself. In this book, Prof. Piaget describes how by imitation the child copies these activities; and in symbolic play represents them in images of his own creation, thus assimilating their reality to his own desires and interests. But there may be no conscious understanding of what these symbols signify. The ego suppresses the knowledge of desires which cannot be admitted to consciousness, or which are perhaps inexpressible because they are too unreal. The egocentric consciousness of the baby, with its lack of differentiation between self and external reality, is not far removed from the semi-consciousness of the adult dreamer. Thus much symbolic play is highly 'magical' in nature, and may include the dream-like activities of kings and robbers, witches and dragons, and so on. The examples which Prof. Piaget gives, symbolizing the processes of excretion, feelings of jealousy and aggression, and anxieties about birth, seem indeed more than normally realistic.

But the child's imitation and play do also bring him gradually into touch with reality, and particularly in the development of language. After pure symbolism there come the animistic myths, familiar to us from Prof. Piaget's earlier books—the moon which goes for a walk with the child, the sun which doesn't need to shine at night because then we are asleep. Later, even this imagery is supplemented and replaced by the realistic imagery which is necessary for obtaining an all-round view of the world, from the point of view of others as well as of the self. Symbolic play is replaced by constructional play and the socialized 'games with rules'. But the use of imaginative symbols and ideas survives in the work of creative imagination, though unconscious symbolism may largely be relegated to the dream.

It is obvious that no more than the briefest outline can be given in a review of Prof. Piaget's argument as to the functions of symbolic imagery and play. But the argument is not clear, and it is far less convincing than that relating to the development from sensori-motor cognition to intelligent thought and understanding. We must continue to value Prof. Piaget's exposition of these latter processes more highly than his grasp of the nature and function of imagination.

M.D.V.

L'Année Psychologique. Ed. H. PIÉRON, A. FESSARD and PAUL FRAISSE. Volume 50. Paris: Presses Universitaires de France. 1951. Pp. xvi+718. 2400 francs.

Vocabulaire de la Psychologie. Ed. H. PIÉRON. Paris: Presses Universitaires de France. 1951. Pp. ix+356. 1300 francs.

This is the fiftieth issue of the *Année Psychologique*, in honour of Prof. Piéron who took over its editorship in 1913. On this occasion nearly the whole content consists of original papers. Many of these are contributed by leading psychologists in various countries who join in paying tribute to the greatest figure in

present-day French psychology. His work is much less known in this country than it deserves. For instance, the *Experimental Psychology*, published in 1927, preserves a freshness to-day which is rare in her contemporary texts.

In an introductory essay Fessard gives an account of Piéron's life and work. Among his books he considers *La Sensation, Guide de Vie* to be the most important.

The main body of the volume presents such an *embarras de richesse* that it will only be possible to outline the wide range of fields covered. Under each broad heading some examples of topics, summarized by the reviewer, will be given together with the names of a few contributors. If more of the writers known in this country are selected, this is merely in order to indicate to the British reader the high general standard.

1. General problems and methodology.

(a) On basic conceptual problems (Lagache, Langfeld, Pillsbury).

(b) On mathematico-logical methods (Burt, Piaget, Thurstone).

2. Physiological psychology.

(a) On sensory functions (Adrian, Chang, Wright).

(b) On nervous mechanisms and animal behaviour (Bennati and Mazella, Fessard and Tournay, Rabaud).

3. Experimental psychology.

On vision and perception (Boring, Gesell, Michotte).

On thinking, remembering and learning (Carmichael, Guillaume, Wallon).

4. Applied psychology.

Occupational (Bartlett, Baumgarten-Tramer, Bingham, Viteles).

On tests and techniques (Elmgren, Katz, Ombredane).

5. Psychopathology.

On clinical tests (Delay and Pichot, Zazzo).

On intellectual impairment (Heuyer, Liberson).

Unlike many symposia, where it is obvious that some contributors have been assigned uncongenial tasks, one gets the impression that the writers of these papers have chosen their favourite topics. The only drawback about this collection is the shock that comes with the realization of how much excellent work is being done on the continent and elsewhere of which one has never heard. For potential readers whose French is not fluent, it should be added that sixteen out of the fifty-five papers are in English.

The *Vocabulaire de la Psychologie* is described by Prof. Piéron as a working tool for people who read psychological literature. It is admirably designed to fulfil this purpose. The language used in definitions is concise. One of the greatest difficulties in compiling a psychological dictionary, namely, the multiple meaning of terms, is satisfactorily handled by listing the different usages and ascribing them to their respective authors. Where they differ from the French, equivalent English and German terms are given. The range covered is very comprehensive, including such recent words as cybernetics, dysthymic, metamorph, narcoanalysis, surgency and tele. Among the few gaps the reviewer has been able to detect are 'social distance' and 'intervening variable'.

A particularly useful feature, absent in Warren's dictionary, is a name index of writers which also contains their terminological contributions. This makes it possible to find rapidly the terms and concepts introduced by, say, Hull—at least in so far as they are incorporated in the dictionary.

In addition, there are appendices providing miscellaneous information, e.g. abbreviations, symbols used in tests and statistical formulae.

Altogether Prof. Piéron and his collaborators have succeeded in producing the kind of dictionary which is badly needed in English-speaking countries, since Warren's is now largely out of date.

Genetic Neurology. Problems of the Development, Growth, and Regeneration of the Nervous System and of its Functions. Conference sponsored by the International Union of Biological Sciences, subsidized by UNESCO. Ed. PAUL WEISS. Chicago: University of Chicago Press; London: Cambridge University Press. 1951. Pp. xii+239. 37s. 6d. net.

The subtitle of this collection of twenty essays by nineteen authors gives a fair impression of what it is about. Problems of the functions of the nervous system receive, however, rather less attention than might be expected, even from neurophysiologists; and much of what there is seems to be related to two

questions. The first is the question of whether 'behaviour develops from the beginning through the progressive expansion of a perfectly integrated total pattern and the individuation within it of partial patterns which acquire varying degrees of discreteness' (Coghill), or whether, as Windle and his co-workers maintain, behaviour is built up by the combination of more or less isolated reflexes. Barron suggests that the former may be true of the axial musculature, and the latter of the appendicular musculature. But the following quotation from Windle (p. 219) suggests that neurologists and psychologists hardly speak the same language; he says, 'A spontaneous mass movement, commonly referred to as "totally integrated behaviour"....'

The other question arises from the large amount of evidence on the specificity and 'selectivity' shown by nerve fibres in their growth, regeneration and activity. For example, it seems that transplantation of a motor nerve to another muscle, at an early stage of development, causes the central connexions subserving the reflexes to rearrange themselves accordingly. The same, *mutatis mutandis*, applies to sensor nerves. Sperry (p. 238) says that in view of such evidence, 'specific nerve energies of the sort conceived by Hering are not ruled out merely because their presence is not revealed by electronic methods'.

There is, of course, a great deal more in the book, but these are the problems most likely to interest psychologists. Those of a wider curiosity will enjoy reading about, for instance, how the young axon is guided in its amoeboid growth along fascial planes and other structural differentiae; and how our old friends the neurofibrils seem to be approaching the status of the wave theory of light—they exist or not according to how you take your observations.

W.E.F.

The Electrical Activity of the Nervous System. A Textbook for Students. By MARY A. BRAZIER. London: Sir Isaac Pitman and Sons, Ltd. 1951. Pp. x+220. 25s. net.

Why is it that text-books for students are so often produced in the most expensive form? This book is a simple, conventional, up-to-date outline of neurophysiology, with the emphasis on electrical phenomena. It resembles in style the relevant chapters of any of the large text-books on physiology. Most students will presumably want it as a *vade mecum* rather than as their sole source of knowledge on the subject, and it can be recommended to those who can afford it. But there is no obvious reason why it should not have been put out in a cheap edition at about half the price. There is only one photograph—a whole-page plate of a squid's giant axon with an electrode inside it; apart from a few knobs and whiskers, it might be a picture of a glass tube with a piece of wire in it, and it is difficult to see what useful information it conveys. The first chapter is quite inadequate as an outline of the physiology of the nervous system, which is what it is called, and it could have been omitted. Much of the rest could have been compressed and clarified by being put in note form and by a greater use of comparative tables. Some readers may like the practice of putting a bibliography at the end of each chapter; as the references in the text are in the vague form of 'Smith found so-and-so' (with no indication of the publication), it probably does no matter. There are one or two contradictions and obscurities, but on the whole the text is clear, accurate and well written. As a parting shot, may it be suggested that electrophysiology is not yet ready to be divorced from its technical methods, and that although, as the author primly remarks, 'the only place to learn technique is in the laboratory', rather more could usefully be said about it in a book of this kind.

W.E.F.

Trends in Gerontology. By NATHAN W. SHOCK. Stanford University Press; London: Geoffrey Cumberlege. 1951. Pp. 153. 20s. net.

A Classified Bibliography of Gerontology and Geriatrics. By NATHAN W. SHOCK. Stanford University Press; London: Geoffrey Cumberlege. 1951. Pp. 599. 120s. net.

The title of the first book is perhaps misleading. In the early chapters the reader is reminded that the average age of the population is increasing, that old people are not employed or housed as often or suitably as they might be and so on. Research establishments and investigations are then listed, but not appraised so that this third of the book overlaps with the Classified Bibliography. All this adds up to the need for a national institute of gerontology in America. Many of the details are parochial, and those who are not the target for Dr Shock's good intentions may well consider £1 excessive for the non-propaganda content.

All concerned with gerontology and geriatrics are likely to find the Classified Bibliography, with its 1,000-odd references, worth the money—than which there could scarcely be higher praise. G.A.F.

General and Social Psychology. By ROBERT H. THOULESS. London: University Tutorial Press. 3rd edition, revised. 1951. Pp. viii+430. 17s. 6d. net.

There will be general pleasure among students of psychology in this country, not only in universities, but in training colleges, tutorial classes and elsewhere, that this eminently readable and interesting book continues to be available. It has been entirely reset, and its new format will no doubt meet with widespread approval. The typography of the third edition is a great improvement on that of the previous editions, while the larger page and the abandonment of such tiresome text-book mannerisms as the use of small print for supposedly more difficult or less important paragraphs makes the book more pleasant to look at and to read.

The teacher of psychology will be mainly concerned to know how thorough the revision has been and how fully new developments and new directions of interest are in fact represented. There are two entirely new chapters: a brief ten-page chapter on the nervous system and brain, and a second chapter on statistics, which includes an elementary outline of analysis of variance. The statistical portion of the book has been considerably lengthened, and the inclusion of examples has made the argument easier for the sophisticated student to follow. These chapters are now a lucid, if brief, introduction to some of the more important statistical methods.

Apart from these changes the modifications are matters of detail, wording and arrangement rather than major revisions. The book is fundamentally the Thouless we already know. Large parts of the book are unchanged or only slightly changed; indeed, there are sections which still remain identical with the 25 edition (wrongly entered as 1935 on the flyleaf). The whole vast corpus of American psychology of the past decade receives only twelve references in a book list of over 300 entries! Contemporary British and European work is rather more generously represented with 18 references. Recent work on instinct (Lorenz, Tinbergen), learning (Hull, Tolman), skill (Bartlett), temperament (Sheldon), sexual behaviour (Fechner), aggression (Dollard, Harding), perception (Postman), conceptual thinking (Goldstein, Heidegger), to mention only a few topics and authors, is wholly ignored. More surprising in a book that is free of bias is the neglect of many of the newer developments and lines of interest in social psychology. While it would be misleading to suggest that the new edition of Thouless has not in some respects been brought up-to-date, it is probably not unfair to assert that the revision has been patchy and unsystematic, and that over considerable areas the more recent modern work has not been taken seriously into account. It will be a matter of regret to many British teachers of psychology that the opportunity afforded by the resetting of this book has not been seized for a much more drastic revision. We rely now in our universities almost wholly on American works for our basic introductory texts in psychology. Thouless has been their one serious British rival. But the American texts are periodically revised with the utmost thoroughness, while Thouless's text, in spite of its new additions and revisions, remains rooted in the psychological milieu of the 1920's and 1930's. His references cluster round the year 1925, while the up-to-date American texts incorporate extensively the work of the past decade, and lead students on, as we want them to be led on, to current research and current controversy.

We shall continue to value Thouless for its terse, readable style, for its shrewd judgements, and for its breadth and sanity of outlook. But as an introduction to many significant developments in modern psychology it can no longer, in spite of its new dress and its ancient virtues, stand comparison with the leading American texts, except in certain limited areas. It will, however, continue to retain its place in the esteem and affections of British psychologists and hold an honoured place among the select company of permanently valuable text-books. L.S.H.

Social Behavior and Personality: Contributions of W. I. Thomas to Theory and Social Research. Edited by EDMUND H. VOLKART. New York: Social Science Research Council. 1951. Pp. ix+338. \$3.00.

There is little doubt as to the value of attempts to systematize both the study of social psychology, and the methods to be employed in obtaining more, and more relevant, data on this subject. Dr W. I. Thomas appears to have been a pioneer in making these attempts. In the course of his writings (the best

known of which is *The Polish Peasant in Europe and America*, written in collaboration with F. Znaniecki. He enunciated a series of general principles governing the development of the individual personality from its innate temperamental basis by interaction with the forces exerted by the various social groups and institutions which influenced that individual. Thomas was particularly concerned to point out that each individual enters an enormous variety of social situations which he 'defines' (i.e. the meanings of which he grasps) in accordance with his inherent and persistent 'attitudes', and with the 'values' which these situations have for him. The attitudes are developed, and the values are given him, largely in virtue of his responsiveness to the customs and codes which have been laid down by society. Thomas then attempted to systematize the various types of social control, the types of reaction to it, and the types of personality which developed. He also studied in detail—largely from autobiographical material—the typical effects upon the personality of change and breakdown in social organization and control. And in the light of the general principles he laid down, he suggested various methods of obtaining further corroborative data.

Now though some of these principles are pioneering efforts which must be modified in the light of later work, nevertheless they do in many respects provide a useful framework for the systematization of this extraordinarily inchoate subject. But it is a good deal more doubtful whether this book employs the best method of exposition of Thomas's ideas. By quoting verbatim various extracts from Thomas's writings (even with fairly extensive explanation and editing) the book inevitably includes much repetition and overlapping, and even contradiction as Thomas modified his own ideas in course of time. Indeed, several of the selections printed in the book are redundant or trivial. For those who wish to read exactly what Thomas said without ploughing through all the original writings, the book may be a valuable one. But for providing a clear idea of his principles, his ideas, his suggestions on method, and the scope of his studies, a general account written by one of his colleagues or disciples might have been more effective. It could also have been made into a shorter—and cheaper—book.

M.D.

A Study of Interpersonal Relations. By PATRICK MULLAHY. New York: Hermitage Press 1949. Pp. xxxii + 507. \$6.50.

This book is a collection of papers previously published in *Psychiatry*, the journal which represents the approach of the Washington School of Psychiatry, founded by Harry Stack Sullivan. It is to honour his memory that these papers have been brought together. The selection of the editor has been judicious; all papers included are of interest and some of exceptional merit. The volume may justly claim to be a fitting tribute to one of the most original and influential of American psycho-analytic psychiatrists.

Sullivan conceived of psychiatry as the 'operational' study of interpersonal relations and shifted his emphasis as a psycho-analyst from instincts and libido theory to object relationships. This emphasis led him into anthropology and sociology, so that he became one of the main originators of the personality culture approach.

It is unfortunate that this more comprehensive social approach was not accompanied by great penetration in depth into the internal world of the individual. For this one must look to Mrs Klein and the London analysts. But in this country psycho-analysis has not developed sociologically to the extent that it has in America.

Apart from Sullivan's own paper, 'Psychiatry: An Introduction to the Study of Interpersonal Relations', of special interest to the present writer are two papers by Ernest Beaglehole on interpersonal theory and the complexity of culture and a series by Clara Thompson on the social pressures and roles of women in Western society from an American standpoint. A wide range is covered, so that the comprehensive nature of the approach inspired by Sullivan can be represented, but a certain vagueness of formulation runs through most of the papers. On the sociological side there is too much 'culture' and too little 'structure', despite the inclusion of a short essay from Parsons. On the psycho-analytic side, though novel insights are abundant, the group of papers on schizophrenic states suggest that a new approach, though diligently sought, has not been attained. There are no new central concepts of a specific kind such as those of the paranoid and depressive positions. In relating psycho-analysis to the social sciences there is always danger of the 'independence value' of the internal world being lost and equally of a certain disregard of the autonomy of social systems. Points of independence and interdependence have both to be appreciated. The studies inspired by Sullivan advance understanding of the problems of interdependence. This was the neglected aspect at the time his thinking matured.

E.L.

Sozialpsychologie. By WILLY HELLPACH. Stuttgart: Ferdinand Enke Verlag. 1951. Pp. viii + 215. D.M. 10.50 net.

Grundriss der Religionspsychologie. By WILLY HELLPACH. Stuttgart: Ferdinand Enke Verlag. 1951. Pp. 212. D.M. 21.80 net.

Sozialpsychologie is described as an elementary introduction for students and practitioners. The first few pages, devoted to a brief exposition of methods, bear some resemblance to the kind of text-book to which we are accustomed. Thereafter the reader familiar mainly with Anglo-American literature finds himself plunged into a world of unfamiliar symbols. This is partly due to the fact that Dr Hellpach's terminology is largely his own; but partly it is accounted for by the different presuppositions on which the book rests. Under these conditions a warning is necessary: although the present reviewer has attempted to follow Dr Hellpach's thought as closely as he could, there may have been some failure of communication for which the reviewer is to some extent responsible.

The book is divided into three main sections under the headings below:

- (1) Ways in which minds affect each other.
- (2) Operative forces and resultant structures of the interaction of minds.
- (3) The reactive influence of the community upon the individual.

The underlying assumption governing the approach to these topics seems to be that social behaviour can be studied by an *a priori* classification of phenomena into more or less plausible units; and that such a edifice needs to be supported only by occasional scraps of empirical data in addition to a wealth of anecdotal illustrations.

Appeal is made to a multitude of sweeping 'laws' or 'principles', which are often so wide as to be unverifiable or tautologous. For example, one of the basic concepts used is an extension of the theory of neo-motor action, the *Ideorealgesez*. It runs as follows: every subjective content of experience includes drive towards its objective realization. No evidence is offered for this and most other 'laws', which appear to have been invented *ad hoc*. Here and there some observation or experiment is casually mentioned, but then it is usually made to carry a top-heavy superstructure of speculation. Thus the *Entwickelungsgesez*, expounded with brief reference to a pencil-dotting experiment carried out by Moede in the nineteen-twenties, is applied to the development of civilizations.

Another instance of a concept being used rather indiscriminately is that of *Individual-Soziale Spannung* (I.-S.S.: degree of individual-social tension). A tension is postulated between a person's 'individual ego' which is limited to pair-relations, and the 'collective ego' which arises in group membership. The following situations are then presented as illustrating the tension:

- (a) A person who is harsh as a boss but friendly and kind in equalitarian social intercourse.
- (b) The behaviour of the average Frenchman in contrast to the behaviour of France (*sic*) as a nation.

There is a delightful discussion here which is of social psychological interest, though not in the way intended by the author. He tells us that whilst the average Frenchman is modest and polite, France has always been inclined to dictate to other nations and to be intolerant. Such a large I.-S.S. is claimed to be typical of most Mediterranean peoples. Germans on the other hand have a small I.-S.S.; they always wish to retain and assert their individuality. Variations in I.-S.S. are probably due, we are informed, to social characteristics.

In his foreword to the revised edition—the book first appeared in 1933—Dr Hellpach insists more than once that he has written a *Tatsachenbuch*, avoiding as far as possible assumptions, theories and interpretations. The present reviewer is forced to the conclusion that this has been achieved by treating speculations, at best tentative hypotheses, as facts. The publisher's announcement states that *Sozialpsychologie* is the only existing German text-book on the subject. If this be true the outlook is indeed disquieting.

Grundriss der Religionspsychologie suffers from a similar weakness of its psychological framework. This is less disturbing, however, as the approach includes sociological, historical and philosophical aspects of religion. The work is well documented, but almost exclusively from German sources; there is nothing to show that the author has come into contact with the thought of Freud, Jung, Durkheim or Westermarck. Psychologists looking for a concise outline of modern theories and studies of religious behaviour and experience are likely to be disappointed; those more generally interested in religion will find some stimulating ideas in this book.

Ritual in Family Living. By JAMES H. S. BOSSARD and ELEANOR S. BOLL. Philadelphia: University of Pennsylvania Press; London: Geoffrey Cumberlege. 1950. Pp. vii + 228. 28s. net.

According to this book there are 1968 professors who give 657 courses on marriage and family problems in 550 American colleges and universities. The stream of voluminous text-books on such topics is thereby explained. By contrast the present work sets out to explore the single theme of family ritual. The concept of ritual is defined as 'a pattern of prescribed behaviour, pertaining to some specific event, occasion or situation, which tends to be repeated over and over again'. The rituals studied are mainly concerning such things as meals or other household routines, collective ways of using leisure time, holidays and religious worship.

Material for the study was obtained from 100 published autobiographies, the reports of 86 university students, a survey of 156 upper, middle and lower class people, and a number of unspecified adults.

The description of family rituals in the past is based chiefly on the autobiographies, that of the present on the students' essays. Both are compared to discover the trends during the last seventy years. Some of the conclusions are merely variations on the well-known theme of the changing functions of the family, many of which have been taken over by other social agencies. The interesting aspect is the emergence of certain compensatory rituals serving to maintain family cohesion.

Class differentials in rituals are analysed in some detail. It appears that continuity with the past is preserved most in the upper and least in the lower class. The latter finds more of its emotional satisfactions outside the family. Middle-class rituals are designed to produce upward mobility. Upper-class rituals centred on perpetuation of status-symbols, are the most formalized.

The view is put forward that, as a rule, families pass through a series of stages from marriage until after the children have left home. Corresponding changes in the nature of rituals are traced.

Throughout the book the descriptive material is interpreted in terms of the meaning and function of various rituals, of which the participants are often unaware. The authors suggest that ritual is the best starting point for studying family culture patterns, and a satisfactory index of family integration.

This last suggestion can be used to illustrate one of the chief weaknesses of the book. There is a rather superficial discussion of the psychological basis of rituals, without any precise statement of the way in which they could serve as an index of family integration. This is not surprising, for an integrated family is somewhat naively defined as a 'well-knit' one. To be of any value the concept of family integration should be given a more clear-cut operational meaning; the characteristics and frequency distributions of rituals would then have to be studied in a more representative population and related to the variables which have been chosen as criteria of integration.

Similar considerations apply to most other topics treated. A variety of interesting hunches emerge from what are often somewhat diffuse data, but few firm conclusions can be drawn.

In spite of this it must be admitted that the general case made out for the significance of rituals is convincing. The authors are perhaps inclined to make too many claims. However, they have provided pointers for a new approach, which is more than can be said of most writings on the family.

Theoretical Foundations of Psychology. Ed. HARRY HELSON. New York: D. Van Nostrand. 1951. Pp. xix + 787. 52s. 6d.

This book 'is designed to treat the fundamental ideas, concepts, theories and problems which are at the centre of the chief divisions of the subject': an urgent but formidable task; a rash title. It is organized in the current text-book fashion not in terms of schools, and the editor states that the seventeen contributors, 'with a minimum of instruction and in most cases with no personal meetings or discussions have succeeded in producing a volume fairly homogeneous as to level and type of material'. One accepts the first part of this statement but disputes the second. To the reviewer, the chapters appear to be planned in the most diverse ways, and written at the most diverse levels. Thus Bayley, 'Development and Maturation', confines herself to illustrating a few broad generalizations. Beebe-Center, 'Feeling and Emotion', concentrates on ordering theories. Bartley examines assumptions in 'Fatigue and Efficiency' and offers a reconstruction of theory. Newman summarizes the historical background of 'Learning', also the facts, contemporary theories, and his own theoretical suggestions. D. B. Klein, 'Abnormal Psychology', deals chiefly with the concept of abnormality. And so it goes on. As isolated papers, many are well worth reading; together they form no kind of pattern. There is little relation even among such intimately con-

ected topics as perception, learning and thinking. Indeed, there is an almost total lack of unity. The theoretical foundations of psychology are sadly elusive.

It may be said that little more is to be expected at the present stage of psychology, even with the most ruthless editing. This is too pessimistic. The attempt to discuss fundamental ideas in some framework other than that of the schools is wholly admirable, but one may wonder whether current text-book arrangements are very much better. Part at least of the answer lies in the excellent section on 'Personality' by MacKinnon and Maslow. To them, personality is the central psychological problem, and an integration of general psychology and the psychology of personality one of our most pressing tasks. This, as they point out, is no more than to take seriously the traditional definitions of the subject. As Hearnshaw says, 'the life-history of the individual man is ultimately the unit with which psychology is concerned'. This surely is the theme which offers hope of a meaningful integration. It is significant that it is only in this chapter (after 600 pages!) that we find any real attempt to discuss the nature of the subject-matter of psychology, and to place the schools in perspective. As a further contribution to unity Krech's lively remarks on 'Psychological Theory and Social Psychology' are also welcome. He argues that general and social psychology are one and the same, and attacks equally the 'asocial' general psychologist and the 'merely social' psychologist. The distinction is indeed one of our wilder aberrations. His review of the perceptual approach to social behaviour offers 'much to hope for the unification of the various fields of psychology'. This book illustrates the magnitude and urgency of the task.

R.B.J.

The Explanation of Human Behaviour. By F. V. SMITH. London: Constable and Co. Ltd. 1951. Pp. ix+276. 18s.

It is good that from time to time a scientist should step back from his own work and that of his nearby colleagues to look more widely over his field as a whole. What he has to say is almost sure to be of value to his preoccupied fellow-scientists. Within the psychological field Woodworth's *Contemporary Schools of Psychology* presents the results of such a survey as does Hilgard's *Theories of Learning*. Prof. Smith's book belongs with these. Not so wide ranging as the first, less specialized than the second, it does not take so much for granted as either, and raises problems of a fundamental kind.

There are really two parts to the book, though its author divides it into three, a theoretical introduction and a set of illustrations from current psychological theory. The division within the illustrations is between a section headed 'Human Behaviour' which includes McDougall, Lewin and Allport, and a second headed 'Three Behaviouristic Systems', those of Watson, Hull and Tolman. The main theories of each author are briefly summarized, but one might, I think, quite fairly ask why McDougall and Watson rather than Freud and Pavlov? But after all these are illustrations; the text lies in Part I. Here Prof. Smith ingeniously combines an account of the problems presented by behaviour with an analysis of the behaviour of scientists studying problems. He refers to Piaget a great deal and to the scientific logicians, but his own purpose is an independent one. Much of what he says is so balanced and shrewd that the reader would be unwise to press his own particular points of disagreement without making very sure that he had a case. I recommend the book not only to psychologists, for the perspectives it may give them and for the enjoyment they will have in reading it, but also and especially to senior students. Woodworth and Hilgard may, one fears, often depute for the writers whose views they describe, and a student may gain the impression that psychology is the sum total of these views. Prof. Smith concentrates his attention upon the activity of psychological thinking rather than its results. You may learn all the psychological theories that have ever been invented and still be no psychologist, but if you can be made to think as any first-year psychologist thought while he was working out his system, then you have made that first step without which you will never really know what psychology is all about.

JAMES DREVER

Problems of Selective Behavior. By JAMES B. KLEE. University of Nebraska Studies. New Series, no. 7. 1951. Pp. vi+119.

The publication of this monograph is a sign of the recent increase of interest in problems of motivation. Mechanistic laws of learning are now subject to widespread criticism, and much attention is devoted to the limitations of the Law of Effect, and to cases in which behaviour, so far from leading to need satisfaction, seems to be utterly inefficient in that respect. Some of the most important work on these problems has been carried out by Maier and his associates, of whom Klee is one, on frustration in rats. It is from these experiments that Klee begins his discussion. Why, when it is faced by a discrimination problem for

which there is no solution, does the rat tend to adopt position habits which become so firmly fixated that it cannot abandon them when discrimination is once more made possible? And why does the human being also persist indefinitely in modes of behaviour which are utterly and completely incompatible with the solution of his problems?

It is evident that Klee has thought deeply on these vitally important questions. He has attacked from a new angle the problems of motivation and reaction to frustration, and the relation of the latter to abnormal behaviour; and has proceeded along lines which may well lead much further towards the solution. Unfortunately, in common with a good many psychologists nowadays, he seems to equal profundity of thought with obscurity of expression. The obscurity is caused by confused arrangements of difficult terminology, and also by sheer bad English.

For anyone who is trying to study the problems of motivation, the monograph will provide many stimulating ideas for further thought; but it will not give rise to any immediate clarification of the present issue.

M.D.

Methods of Psychology. Edited by T. G. ANDREWS. London: Chapman and Hall. 1949. Pp. xiv + 716. 30s. net.

The Energetics of Human Behaviour. By G. L. FREEMAN. London: Geoffrey Cumberlege Oxford University Press. 1949. Pp. 344. 20s. net.

Individual Behavior. By DONALD SNYGG and ARTHUR W. COMBS. New York: Harper and Brothers. 1949. Pp. ix + 386. \$3.50.

In *Methods of Psychology* twenty-two contributors each write a chapter, the aim being to produce a text which lays emphasis on methodology and which may be used as a text-book in a second-year course in experimental psychology. The first chapter, by the editor, deals with methodology in general; the remaining chapters, each by a specialist, aim at displaying the methods used in a particular sphere.

In so far as it is difficult to discuss methods without becoming involved in theory, the result is not greatly different from the standard modern text-book. It is more likely to be of value to the student as a supplement to an older text than to be studied with a view to learning about methodology.

Anyone who considers that psychology is no longer divided in outlook should read in turn *The Energetics of Human Behaviour* and *Individual Behavior*. The first is in the behaviouristic tradition and contains a telling attack on phenomenism. The second is in the Gestalt tradition and is in the main a plea for phenomenism.

In *The Energetics of Human Behaviour* an attempt is made to increase the scope of objective behaviour study by the use of covert indicants of behaviour such as the E.E.G. and P.G.R. The principle of homeostasis is extended so as to apply to 'total behaviour' a number of constructs being introduced to bridge the gap from the physiological level. There is also much interesting discussion and criticism of various theoretical issues such as the Mind-Body problem, phenomenism, behaviourism and eclecticism.

Individual Behavior states the case for phenomenism. All behaviour is considered as determined by the phenomenal field of the organism. In order to predict behaviour of another person it is necessary to reconstruct his phenomenal field by observing his behaviour, and then to project his future field and thence his consequent behaviour. This operation is stated to be of the type 'Under what circumstances would I have done that'. Knowledge of the subject's phenomenal field is gained by inference from his behaviour and the assumption that there are common features in the phenomenal fields of the observer and the observed.

The nature of the phenomenal field is discussed in terms of fluidity, organization, figure and ground. Thence the ability to reduce learning, problem-solving, reasoning and remembering to a single process conforming to common principles is demonstrated, i.e. learning—differentiation of the field, remembering—differentiation of past aspects of the field, etc. The second part of the book is concerned with applications of this type of approach to problems in education, therapy, diagnosis and research.

C.E.M.

Gestalt Psychology. By DAVID KATZ. London: Methuen and Co. Ltd. 1951. Pp. viii + 171. 12s. 6d. net.

This book is a translation from the second German edition of a work which first appeared in Swedish in 1942 and is the second in the series, Methuen's Manuals of Modern Psychology. There are twenty-nine usually very short, chapters and over thirty clear diagrams.

Prof. Katz indicates in several examples how the concepts which have been associated with the writings of the Gestalt school provide a justifiable addition to the earlier 'atomistic' approach in the understanding of many experimental results; but perhaps the most refreshing feature of his treatment is the frank manner in which the too facile application of Gestalt concepts, and the failure of the protagonists of this school to extend them to some clearly appropriate fields, are criticized. In this respect, the chapters entitled 'Gestalt Laws of Mental Work', 'Connective Inhibition and Mental Dazzle in Thought Processes' and 'The Transposition of Action Forms' are particularly useful and provide at the same time some accounts of Prof. Katz's own researches.

As the author indicates, Gestalt concepts may be applied in many branches of psychology; but the amount of information he offers and the continuity of his treatment in several chapters are inadequate for the purpose of a manual. The student who is already acquainted with the speculation on fundamental issues to be found in W. Köhler's *Gestalt Psychology* (first published 1929) and the clear, well-documented treatment of the many ramifications in G. W. Hartmann's *Gestalt Psychology* (Ronald Press Co., 1935) would, however, find the author's observations of value. They derive from a long association with and many contributions to the Gestalt movement, suggesting that perhaps a more appropriate title might have been, 'Reflections on Gestalt Psychology'.

F.V.S.

Psychology. By NORMAN L. MUNN. 2nd edition. Boston: Houghton Mifflin. 1951. Pp. xvi+624. \$4.50.

The second edition of this widely used text-book fulfils the need for an introduction to psychology from which the student can effectively proceed to a more advanced level. The author has rewritten and revised much of the original text to incorporate the more important findings of the last five years, notably in the discussions of learning, motivation, emotion and individual differences. Although there has been no alteration in the general arrangement, the book achieves a better balance than the first edition in its treatment of the subject. Among the many illustrations there are a number of new ones, but a coloured plate has been misplaced in the first printing.

S.B.N.S.

Personality in Theory and Practice. By A. A. ROBACK. Cambridge, Mass.: Sci-Art Publishers. 1950. Pp. 437. (No price given.)

This book is a much expanded version of the same author's well-known book *Personality; the Crux of Social Intercourse*, with much new and additional material. It is divided into two parts, the first dealing with the approaches to the problems of personality from various angles and by various methods, and the second part dealing with problems of the applied psychology of personality. There are two appendices, a good glossary, an extensive bibliography, an index of names and an index of subjects, and five illustrations, most interesting of which is a beautiful portrait of William James, hitherto unreproduced.

Prof. Roback has distinguishing gifts which make themselves felt strongly in this work, and will commend it strongly to all beginners in psychology and to not a few old-timers in addition. Most outstanding of these gifts is the ability to express difficult and even technical matters in clear, flowing and readable language. As he says himself, we do not need to resort to 'Newspeak' in order to write scientifically. Next comes great fairness and objectivity of outlook, the ability to see in the solid and in perspective, so that views taken from one side or angle do not seem like the whole picture. It is therefore a pleasantly refreshing, illuminating and insightful book, and should have a wide appeal to thoughtful readers among the general public as well as among students of psychology.

R.W.P.

Emotional Security. By M. R. SAPIRSTEIN. London: Rider and Co. 1951. Pp. xii+291. 16s. net.

The keynote of this enlightened work is summarized on the last page: 'Psychoanalysis based on a temporary and impersonal contact between physician and patient within an analytical office can never substitute for healthy and helpful relations in real life.' It is clearly written and should be of value to the general practitioner or educated lay reader. There are three sections. The first part deals with the application of Freudian teaching to breakdowns which lead to a failure in adaptation. The author explains that there are multiple factors involved, including changes in environment and physical illness or exhaustion:

'Leaving out the disturbances in adaptation which are decisive for survival, such as eating, breathing etc., the one defect which universally causes anxiety, in all cultures, is the loss of capacity to make effective contact with other human beings—in effect, isolation.' He points out that there are three basic defences against such anxiety—withdrawal, hostility or dependency. He does not deal extensively with analytical techniques, but observes: 'If the analysis is exclusively devoted to making unconscious feelings conscious, that is, through symbolism and dreams, it becomes interminable.' This epoch has been called the age of anxiety, and the writer stresses the importance of protecting the child in its early years. In his own words: 'The so-called "democratic" relationship between parent and offspring is essentially a rejection to the child, since it implies equality when it is non-existent. Actually it constitutes a parent's avoidance of responsibility and leadership when it is so sorely needed by the youngster. It is no free accident or change in diagnostic skill which accounts for the decreasing number of hysteric (dependent) types and the increasing appearance of schizoid, detached personalities. The apparent self-sufficiency of man is beginning to boomerang!'

The second part is concerned with sexuality from various angles. Much of the ground he covers on this has been already well tilled, and he gives no answer to the recurrent question of 'how much extramarital sexual feeling or activity is normal?' It is somewhat surprising to find that 'the concept of romantic love is a recent one in our culture and reaches its highest form on the American scene'. This is paying scant tribute to the Troubadours or romantic love of Dante, Abelard and Petrarch. There is a good deal of sense on the reaction of women to marital life; the old simile of the husband being like an orang-utan playing the violin makes one wonder if the modern American still suffers from his Anglo-Saxon inheritance. Frigidity in women can be due to anxiety, but this is not its only cause. I agree that what he politely refers to as 'heavy sexual play in adolescence' must be a bar to rather than a preparation for marriage. It seems a little out of date to refer to the educated woman as being less able to love than the pure feminine one. In my experience the gifted personality is often the most fulfilled in all human relationships.

Part three is rather too much condensed; it considers only a few special applications of therapy, advising against analysing an artist and discusses very briefly psychosomatic symptoms. There is a truly colossal bibliography, but the author speaks with sufficient authority of his own.

STELLA CHURCHILL

Psychotherapy. By PAUL SCHILDER. Enlarged and revised edition arranged by LAURETTA BENDER. London: Routledge and Kegan Paul, Ltd. 1951. Pp. xiii + 396. 30s. net.

The first edition of this book appeared in 1938. The present edition contains some additional material gleaned from articles written by Dr Schilder before his untimely death in 1940, and selected references to more recent work in the form of footnotes and an extended bibliography. This revision has been most competently done by Dr Lauretta Bender.

Schilder is remembered as that *rara avis*, a neurologist who took psycho-analysis seriously. Although the emphasis in the present book is predominantly psychological, there are abundant reminders of the author's earlier concern with organic neurology which do much to set his account of the theory and practice of psychotherapy in proper perspective. For example, he has some interesting comments on the affinities between obsessive-compulsive illness and post-encephalitic syndromes which suggest an organic background in some types of psycho-neurosis. More important, perhaps, is the fact that he has no doubt whatsoever as to the organic and constitutional basis of the so-called functional psychoses, and is rightly cautious about the possibility of psychotherapy in such conditions. As he slyly remarks: 'It is rather amusing that one has in these days to defend the attitude that there is anything organic in organisms.' None the less, it should be borne in mind that Schilder's understanding of the theory and practice of psycho-analysis was very considerable, and it would be entirely wrong to dismiss him as a reactionary survival from the pre-Freudian era. Indeed, in his attempt to combine the teachings of organic neurology with the standpoint of dynamic psychology he was undoubtedly ahead of his time. The last-ditch defenders of a purely psycho-analytic psychiatry have much to learn from his integrative approach to the problems of mental illness.

This book will be of interest and value to medical psychologists and others concerned with common problems in neurology and psychiatry. Despite its title, however, the approach is predominantly theoretical, and the book cannot be recommended as a common-sense guide to inexperienced practitioners of the psychotherapeutic art. Further, some of the questionnaires advocated for use in group therapy provide abundant ammunition for those with a taste for tilting at psychiatric solemnity. But lack of humour does not necessarily imply lack of wisdom, and this new edition of Dr Schilder's book will remain as a fitting memorial to a very remarkable man.

O.L.

The Hero with a Thousand Faces. By JOSEPH CAMPBELL. The Bollingen Series, xvii. New York: Pantheon Books, 1949; London: George Allen and Unwin, 1951. Pp. xxiv + 416. 25s. net.

This book is most refreshing. It tells the story of the hero, one of the most prominent archetypes and a very frequent figure in mythology. The author contends that the study of mythological parallels, compared with the interpretation of dreams as personalized myths and supported by the psychological insight of the analyst, provides the grammar of the life symbols and the key to their mysteries. The modern physician is the master of the mythological realm, the knower of all the secret ways and words of potency. His role is that of the wise old man of the myths and fairy tales, whose words assist the hero through the trials and terrors of the weird adventure.

The author shows that the hero is the man of self-achieved submission; but submission to what? In various chapters he demonstrates convincingly first the call to adventure or the vocation of the hero, then the refusal of the call or the folly of the flight from the God; next, the supernatural aid, the unsuspected assistance that comes to the hero who has undertaken his proper venture; then the crossing of the first threshold, and the belly of the whale, that is, the passage into the realm of the night. Then follow the stages of the trials and victories of initiation, starting with the road of trials or the dangerous aspect of the gods, the meeting with the goddess (Magna Mater) or the bliss of infancy regained; woman as the temptress, that is, the realization and agony of Oedipus; the atonement with the father, the apotheosis and the ultimate boon.

Like all other books of the Bollingen series, this book is beautifully produced and its value enhanced by numerous illustrations and plates. Unfortunately, they are sometimes inadequately described, both in the list and in the text; nor are they always explicitly referred to in the context. These are minor faults, however, which should not deter those interested in analysis and in spiritual life from studying the book closely.

L. STEIN

Man into Wolf. By ROBERT EISLER. London: Routledge and Kegan Paul. 1951. Pp. 286. 21s. net.

Very wide and varied reading, a lively, speculative mind, and great versatility are evident in this work by the late Dr Eisler. Only the very naïve, however, could take it seriously as a scientific study. It offers another excursion of fantasy into the realms of prehistory, comparable with Freud's tales of the primal horde, but orientated by an unquestioning belief in Jung's archetypes. The story is that men were originally harmless frugivorous animals living in peaceful herds, but that at the end of the pluvial period hunger drove them to imitate gregarious beasts of prey by becoming carnivorous hunting packs, preying on animals and the more retarded herds of fellow-men who had not made the changes from man into wolf. With the change came the introduction of aggressiveness and violence into sexual practices. Every historical and anthropological example of human hunting packs, from the maenads to the Moroccan *Isâwiyya*, and any practices that may lie behind the legends of wer-wolves, are treated by the author as activations of the archetype established through the original formation of human hunting packs, and handed on by Lamarckian processes.

It is also a racial memory of our peaceful life among fruit-bearing trees that makes some of us enjoy walking in woods and explains 'why we can still feel tempted by a dessert of fruit at the end of a lengthy dinner when nothing could induce us to start on another dish of meat or fish'.

Far-ranging notes on the human impersonation of hunting animals and on allied topics provide a striking display of erudition but give no scientific support to the author's central assertion. Yet those who are interested in these forms of human rapacity and their relation to warfare will find a useful assemblage of material and sources in this book.

Traditional Number Rhymes and Games. By F. DOREEN GULLEN. Publications of the Scottish Council for Research in Education, xxxii. London: University of London Press. 1950. Pp. x + 144. 5s. net.

Miss Gullen's intention in making this collection was 'to supply teachers with informal number material natural and agreeable to their youngest pupils, and to try to recover for the pupils some of their heritage of Scottish number rhyme which threatens to be lost in the rush and jostle of modern life'. The material

she has brought together is by no means exclusively Scottish, a great part being a familiar part of the English tradition too. The result is an agreeable collection of counting-out rhymes, riddles, rhymes for ball-bouncing, and other games, and many familiar nursery rhymes that make some reference to numbers (among them, of course, cumulative rhymes). The net has been cast wide, to take in reference to hours, days of the week, any division of the calendar, and 'festivals', the last section allowing the inclusion of many rhymes referring to farm work and the weather. A few charms (e.g. for curing hiccups) also appear.

Apart from the use these rhymes may have in creating in infant pupils 'a pleasurable attitude to number' (as the Preface hopes), they are a valuable reminder of the tradition of folk rhyme that they represent, and at several points have special psychological interest: for instance, the conversion of adult interests (such as work, courtship and marriage) into children's terms, the apparently higher proportion of rhyming patten in girls' games than in boys', and the end-result, in the nonsense of many rhymes, of an evident process of serial reproduction from material that was once meaningful. It is not clear how many of the rhymes were collected directly from children and how many are reprinted from existing collections, but the book makes no claim to the scholarship that would make this question important.

Advertising Psychology and Research. By DARRELL BLAINE LUCAS and STEUAR HENDERSON BRITT. New York, Toronto, London: McGraw-Hill Book Company Inc. 1950. Pp. xii + 765. 55s. 6d. net.

The authors are psychologists with both university and business experience, and the book is designed as a text for use in colleges, advertising agencies and industrial concerns. For the American student it would appear to be an excellent guide book to a comprehensive study of the psychology of advertising. In addition to footnote references to original papers, there are reading references to more than sixty standard works dating from 1930 to 1950, with Poffenberger (1932), Burr (1938) and Hepner (1949) as a foundation. Its excellent glossary is a valuable tool, and the student who works with understanding through the twenty-six pages of questions and exercises should have a good scientific grasp of fundamentals. The book should prove valuable to the British student and advertising practitioner, especially as giving reference to the more recent work in America.

Its twenty-three chapters are divided into six parts. The first four parts show how the advertiser seeks to catch, hold and use attention and how this involves psychological knowledge of interest, comprehension, belief, feeling, memory and motivation. In these parts constant reference is made to research work, and, in the last two parts, the methods of advertising research are brought together and explained critically.

That an advertisement fails unless it makes a lasting useful impression is stressed, and that this involves an understanding of the fundamental motives of consumers. 'To get an advertisement noticed is easy—as easy as it is to get attention by whistling at a concert. . . . But to get an advertisement *trusted*, believed, acted upon. . . is. . . hard gruelling work.' 'We have an enormous quantity of *marketing facts*, but unfortunately we have too little *psychological information* about consumer motives.' The need to elicit accurate information by personal inquiry is great, but 'too much interviewing has been done by people who are otherwise unemployable'.

Much space is given to radio and television advertising. This, which is of little practical value here, may interest our practitioners. Radio advertising is now second to newspapers only. It has been running for twenty years, and the advertising use of television has grown rapidly since the war. Some ideas can be expressed more clearly through the voice, and it seems that radio is particularly effective at lower economic levels. 'The sponsor of a radio program has so much of an advantage in getting attention in advertising that his messages are often resented by his listeners.' To secure knowledge of the extent to which radio sets are used a number of methods have been evolved. Chief among these are the 'Gallup diary method' by which a written record is made at the time of switching on, 'Hooperatings' obtained by ringing up selected telephone numbers and inquiring whether the radio is in use, and, if so, on what station, and the 'Nielson mailable-tape audimeter' which, attached to the receiver, records time of use and station. While the value of tests is emphasized, the sources of error to which they are liable are noted and the ways in which compensation can be made for some of these errors are carefully explained. The book is labelled 'an Introductory Book' bears witness to the advanced state of the psychological study of advertising in America.

The Teaching of Statistics in Schools. A report of the Council of the Royal Statistical Society. London. 1951. Pp. 12. 1s. net.

This report reviews the widespread use of statistical techniques in industry, science, sociology and economics and of quantitative data in a wide field of matters which touch the daily life of citizens, drawing attention to the need for a critical understanding on the part of the public, of the reliability of numerical data, particularly when the material has been obtained by some sampling method. A strong case is presented for the general introduction of statistical work in all secondary schools. The more specific needs of pupils of technical and commercial schools and of the sixth forms of grammar schools are also considered.

The report then discusses the place of statistics in the curriculum, advocating statistical treatment of suitable material as it occurs in biology, geography, history lessons, etc., rather than the introduction of a new subject, 'statistics', as an independent entity in the time-table. This implies an arithmetic course enlightened by an understanding of the nature of statistical methods and a critical approach to the sources of arithmetical data. It recognizes the shortage of teachers able to conduct such courses and the need for suitable text-books. This need has been felt in schools for some time, and, whilst it is encouraging to read that 'one or more books are in preparation', it is disappointing that the Royal Statistical Society itself offers no bibliography of suitable books. It is to be hoped that this report will stimulate some experienced teachers to meet this need.

Finally, the report quotes a number of existing General Certificate of Education syllabuses.

This report is modest and realistic in its recommendations and should interest those concerned with school curricula and examinations and with the training of teachers.

F. W. LAND

Introduction to Statistical Method. By B. C. BROOKES and W. F. L. DICK. London: Heinemann. 1951. Pp. viii + 288. 21s.

This book, which is in two parts, is written as an elementary text-book for schools. The first part (which is available separately) deals with the tabulation and graphical representation of numerical data, the calculation of various types of average—called here 'measures of position'—and measures of dispersion; probability and descriptions of the Binomial, Poisson and Normal distributions. In the second part are described tests of significance, regression, correlation and χ^2 . The book finishes with a chapter on planning statistical experiments. For the most part the writing is clear and straightforward and should be within the grasp of older school-children, although to follow the book fully, familiarity with sixth-form mathematics is essential. An excellent feature is the profusion of examples and exercises with answers. These provide adequate practice in all the methods described.

Occasionally the symbolization is a trifle confusing, due to a laudable attempt to keep population parameters and sample statistics distinct, and it seems rather a pity to teach students to call the Spearman rank correlation coefficient ' R '—a symbol which they will find stands for a multiple correlation coefficient in most other places. No mention at all is made of the Kendall rank correlation coefficient, a statistic which is proving itself to be very useful, particularly to research workers in psychology. It is also regrettable, one feels, to teach students to use a rather makeshift method based on the standardized range for examining the homogeneity of a set of means. Anyone who can cope with the second part of this book can certainly deal with the simpler analyses of variance. A few errors have slipped through the calculator's net. For instance, at the foot of p. 165 'eighteen thousand pounds' should read 'eighteen thousand, five hundred pounds'. On p. 236, Example 3, second paragraph, the second sentence should begin, ' μ theoretical class frequency should be less than 10'; and on p. 262, fifth line, the divisor should be $\frac{1}{10}$ and not $\frac{1}{16}$.

In spite of these minor blemishes, however, the book is to be recommended as a sound introduction for intelligent students. The wealth of practice material is particularly useful.

E.G.C.

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This book, which was reviewed in the *Journal* for November 1951, has now been issued by an English publisher.

